The Mighty Force of Research

BY

THE EDITORS OF *FORTUNE*

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Preface

In the last fifteen years the U.S. expenditure on research and development has climbed from about $900 million to some $4 billion annually. Roughly 95 per cent of the $4 billion goes into exploiting previous scientific breakthroughs; perhaps 5 per cent into expanding the storehouse of fundamental knowledge. The 95 per cent gives us new antibiotics, polio vaccines, color television, power steering, jet transport planes, and atomic weapons. The 5 per cent helps us to understand the forces inside the atom, the structure of the universe, the functioning of the human brain, and the nature of life. Taken together, the two aspects of research—the pure and the applied—are the mightiest force in the U.S. economy, as the title of this book suggests.

It is tempting to omit the qualifying word "economy," for it is difficult to think of an aspect of modern life that does not bear the strong imprint of science and technology. Indeed, some have argued that the imprint is too strong and that what is needed is a moratorium on research to give the world a chance to collect its wits. In the nuclear age, the argument has a certain appeal. The fact is, however, there never was a "safe" plateau on which man could pause, even if he wanted to. Nor has any plateau ever been high enough to satisfy man's aspirations. Certainly the present one is not. Too much of the world is short of food, water, energy, medical care, housing, and laborsaving machinery to call a halt now. To be sure, research on the frontier does not meet these wants directly. But without forward-looking research there will be no pesticides, no atomic power plants, no solar batteries, no means

for desalting sea water, and no transistors for automatic control of machinery.

In the U.S., which owes its high standard of living to technology, research has become an inseparable part of the economy. Recently Raymond Ewell of the National Science Foundation tried to answer the question: How much does society get back, in dollars and cents, for its investment in research? His astonishing answer: 100 to 200 per cent a year over the last 25 years. In other words, society has got back \$2,500 to \$5,000 for every \$100 spent on research and development. Ewell obtained his figures by estimating the portion of the gross national product of the year 1953 that could be attributed to the research achievements of the preceding 25 years.

Ewell also finds that research investment has been growing at an exponential rate. Between 1776 and 1954 the U.S. spent nearly \$40 billion on research—half being spent since 1948. The present research investment of about \$4 billion a year represents 1.1 per cent of the gross national product. Ewell predicts that the research outlay will exceed \$5.1 billion in 1960 and reach \$6.3 to \$6.9 billion in 1965.

This book provides a broad-scale look at research in America as *Fortune* has observed it over the last few years. The fifteen articles reprinted here appeared originally in *Fortune* between January, 1953, and August, 1955. The book is divided into two parts. The first five chapters provide a general introduction to the central topic. The first chapter is a searching appraisal of the U.S. research effort in the decade 1945–1955. The second chapter, The Young Scientists, introduces the reader to the types of young men who succeed in scientific research. They are an unorthodox and freedom-loving lot. The third chapter discusses the contribution of a leading independent laboratory, Arthur D. Little, Inc., which has grown up with U.S. research. Chapter 4 describes the activities of the long-range planning group in America's premier industrial

research organization, Bell Telephone Laboratories. Chapter 5 presents the plight of the lone inventor, engulfed by the fast-growing complexity of modern technology.

The second part of the book focuses down upon specific problems and areas of inquiry. Here it is seen that U.S. researchers have performed a leading role in some of the most dramatic advances of the last decade. They have helped develop new concepts of brain function, devised plastic membranes for purifying salt water, discovered cloud seeding, applied electronic computers to weather forecasting, brought modern chemistry to the aid of the farmer, opened up a new era in metallurgy, extracted power from the atom, conceived the transistor, and automatized factories.

The book concludes with an account of one of the most brilliant theoretical concepts in modern science: the Information Theory, developed by Norbert Wiener of Massachusetts Institute of Technology and Claude Shannon of Bell Telephone Laboratories. The theory addresses itself to the measurement of that most essential and ubiquitous of commodities: information. As one might suppose, the theory is immediately useful to telephone, radio, and television engineers (since it tells them precisely how inefficient their present systems are); it promises also, however, to be of increasing value to linguists, biologists, neurophysiologists, psychologists, and others who must deal with more subtle types of communication networks.

The fifteen articles are reproduced essentially unchanged. Although Chapter 11, The Peaceful Atom, was written before the Geneva Conference of August, 1955, it was already clear at the time of the writing that many long-held "secrets"—highly technical ones—would be divulged at the Conference, and that they would not fundamentally alter the outlook for peacetime atomic power, as presented in the article.

The most striking instance in which an article antedated a momentous development in its field is the one on solar energy (Chap-

ter 10), which appeared in September, 1953. Some eight months later Bell Laboratories announced its solar battery, the first promising device for converting solar energy directly to electricity. The article had pointed out that such a device "would certainly become the world's greatest convenience if it could be made to produce currents in large quantities and efficiently." The Bell System lately began using the solar battery to provide power for rural telephone service, but the long-range prospect of utilizing solar energy—as set forth in the article—has not yet been greatly altered by the battery's arrival.

For assistance in preparing these articles *Fortune* is indebted to many scientists and engineers in government laboratories, in universities, and in industry. The list would easily exceed three hundred names. For their pains none ever received more than a letter of thanks and a complimentary copy of *Fortune*.

The authors of the book's fifteen chapters are credited individually on the title pages. All but two are present *Fortune* editors. Lawrence P. Lessing, an editor at the time he wrote Chapter 12, is now a free-lance writer. Also a free-lance is Morton M. Hunt, author of Chapter 4.

Fortune researchers who worked on these articles include: Marjorie Jack (Chapters 2, 6, 7, 8, 13, 14, 15); Ruth Miller (Chapters 9, 10, 11): Marion Heimlich (Chapters 1, 9); Eileen Durning (Chapter 3); Edith Roper (Chapter 5); and Betty Fullen (Chapter 12).

For its 1953 science and technology articles—seven of which appear here—*Fortune* was cited "for distinguished science journalism in magazines" by the American Association for the Advancement of Science. Two 1954 articles, The Young Scientists (Chapter 2), and The Inventor in Eclipse (Chapter 5), received citations for excellence from the 1954 Benjamin Franklin Award Committee.

The Editors of *Fortune*

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The Strange State of American Research

BY ERIC HODGINS

RESEARCH IN AMERICA today is a fat boy. It feeds on billions generously supplied by government, industry, private-foundation funds, and the universities. And the fat boy's muscles are hard, too; there is nothing slow or slothful about the way research in the nation's great laboratories is planned, assigned, or carried out. The fact remains, however, that the enormous body of scientific research in America is suffering from a kind of malnutrition—and this in turn has serious implications for the future of American greatness.

The idea of science going hungry in a country so technologically rich as our own seems at first glance absurd. Science in America is indeed surrounded by every resource and trapping that a gadget-happy nation can provide. The malnutrition, then, is not due to scarcity; it is due to something more serious, a systemic condition in American society that hinders science from utilizing the nutriment at hand. The trouble seems to be that we have yet to provide all the elements necessary to create and maintain an intellectual atmosphere in which *fundamental* scientific research can flourish as it should. And this is serious. For the state of research has a sharp bearing on the prospects for further economic advance in the U.S. This bearing is neither direct, immediate, nor predictable, and any-

one who demands that it should be misses the whole importance, to Man, of his inquiry into the ways of Nature. But there are also many misunderstandings about the nature of research that interpose between us and our future a heavy barrier not yet breached.

A crucial distinction must be recognized at the outset. "Science" is not the new nuclear reactor, the miracle drug, the TV set, or the washing machine. These things are technological end products only, and in their development America is unquestionably supreme. But if science is to be found in them it is only by puzzling back through a mystic maze until, behind designers, engineers, laboratories, and "applied" scientists, there is found at last in the center of the maze the *fundamental* scientist; the Research Man; the man whose life and work *is* science—the man who is doing "uncommitted" thinking or experimentation, "prompted by disinterested curiosity and aimed primarily at the extension of the boundaries of human knowledge." *

Fundamental research is the fountainhead of science, pure and applied, and the wellspring from which technology and engineering dip. And in "science"-worshiping America there is not nearly enough of it. As scientific administrators stand and survey their teeming scene, they are at once optimistic and dissatisfied; dedicated to their propositions, but also dubious. Things were never better—but they are not good enough. Our wealth of scientific gadgets and our vast organization of scientific projects are in heavy disproportion to our depth of scientific thought. We "research the hell" out of everything; we *contemplate* very little.

The citizen-at-large, along with the scientist, ought to be deeply concerned about this, for much more than his economic welfare may be at stake. In a statement published a few years ago, Arnold J. Toynbee predicted that three hundred years from now this

* *Definition from* Sponsored Research Policy of Colleges and Universities: *A Report of the Committee on International Research Policy, American Council on Education, 1954.*

bloody twentieth century will be remembered not for its splitting of the atom, not for its diminutions of distance and disease, not even for its shattering wars—but for "having been the first age since the dawn of civilization . . . in which people dared to think it practicable to make the benefits of civilization available to the whole human race." It is in America, almost exclusively, that this bold idea has begun to be explored. All of America seems not yet willing to commit itself wholeheartedly to the proposal, which has scarcely yet come formally to its notice. The uncertainty of Americans about their own (and hence the world's) future was summed up by two scientists high in the administration of science and research in America. Said the first, "There is no question in my mind that within the next twenty-five years America will become the intellectual center of the world." Said the second, thinking of McCarthyism and McCarranism, and other blots on the national record, "Then, by God, we'd better get started!"

The heritage

As of today, it can be said that modern basic science and research in America is twenty-five years old—still at once callow and mature for its age. It is just twenty-five years since the severing of the umbilical cord to Europe through which American science, for more than a century, drew its intellectual sustenance.

The first American ever to be honored with a Nobel Prize was Albert Abraham Michelson, who received it in 1907 for work in physics begun many years earlier in Cleveland. The list was slow in lengthening, for American science remained almost completely beholden to the great minds of Europe. But a decade before Michelson's recognition the lines for future native greatness were being laid out. Before the turn of the century young Americans of intellectual promise were making their pilgrimages to Europe in greater and greater numbers, to sit before the great scientific minds in which England, France, Germany, and Scandinavia then

abounded. They came home inspired—by Sir Ernest Rutherford, by J. J. Thomson, by Röntgen, by the Curies, by Max Planck—and on their return they began to make felt in America an influence that had scarcely stirred before. In turn, they became the teachers and inspirers of still younger men who then made *their* way to Europe—men like I. I. Rabi, Edward U. Condon, Marston Morse, and J. Robert Oppenheimer. These younger Americans also were inspired by the Rutherford generation, and by other brilliant scholars like Niels Bohr, the great Danish propounder of the quantum theory; by that inspired teacher of theoretical physics, Arnold Sommerfeld in Munich, and that trio of profound mathematicians, Heisenberg, Schrödinger, and Dirac.

It was from contact with such men as these that the young Americans at last imported into the U.S., on a going basis, the standards of European physical science: its rigor of thought, its soaring imagination, its terrific iconoclasm about the Newtonian models of the past. Thus it was in the 1930's, when everything else was going down the drain and American industry was so prostrate that many thought it would never rise again, that American physics came of age. (Europe's *émigrés,* who made such an enormous further contribution, came later still.)

"When I first went to Europe a quarter of a century ago," says Professor Rabi, "I was provincial; when I went to Europe after the war it was Europe that had become provincial." What had happened, of course, was that Europe's intellectual life had been shattered by the war, while ours, particularly in Professor Rabi's specialty of physics, had been vastly fostered and nourished. Today, the growing power of American scientific thought is a prime fact in our lives, but a fact of which we have yet to take the best advantage. The distinction today between Europe and America in the physical sciences was put this way by one scientific administrator: "Let's divide all science and scientists into two groups: call 5 per cent 'the top' and the other 95 per cent 'the rest.' Our American 95 per

cent is much better than Europe's 95 per cent—but Europe's top 5 per cent still surpasses ours."

$4 billion worth of what?

The bill for "research" in America now reaches almost $4 billion yearly. But this massive total lumps everything together. It lumps together routineering and genius. It lumps together the fundamental and the applied. It lumps together the contributions of government, industry, the universities, the foundations, and all sorts of miscellaneous institutes organized to the ends of "science." And finally it lumps together all the *kinds* of science there may be thought to be: the "physical sciences" (mathematics, physics, chemistry); the "life sciences" (biology, medicine, agriculture); the "earth sciences" (geology, geography); the "behavioral sciences" (psychology, sociology, anthropology). It is the physical sciences that account for the great size of the figures, and it is the status of fundamental research in these physical sciences with which we are chiefly concerned.

What is the *value* of research in America today? The answer is, of course, that no value can be placed upon so vast a thing as inquiry into the fundamental secrets of nature. The temptation in this country is almost overwhelming to justify every human activity in terms of its immediate usefulness or efficiency. But the yardstick of instant usefulness does not properly measure research. Most great thoughts, most basic observations, begin by being "useless." The Newtonian physics once belonged in this category. So did the Mendelian laws of heredity. So did the Bohr atom. So did quantum mechanics. So did the equivalence of mass and energy. The greatest danger to American science, and to the whole concept of research in America, is its constant confrontation by the "show me" boys whose mentalities cannot encompass what it is all about.

Today, among its many virtues, American research carries a dangerous recessive gene. Our talent for organization and our vast

resources accomplish great things in areas to which these good fortunes can be made to apply, but they stifle and stultify certain expressions of genius that are more apt to thrive in an atmosphere of lonely reflection and austerity where little equipment is needed beyond pads of memorandum paper and a batch of well-sharpened pencils. "But you don't understand," recently said one defender of things-as-they-are; "what we've done in this country is to be able *to conduct research by means of the average man.*" This is indeed an accomplishment, but it has its dangerous side. Where once the world of science, in ancient and medieval days, was all thought, theory spinning, and speculation, with no experiment at all, today research in America has swung too far in the other direction: too much experiment in relation to the amount of thought. So long, says J. A. Gengerelli, head of the department of psychology at U.C.L.A., and author of *Facts, Thoughts and Dreams*, as we keep on financing larger and more elaborate and precise instruments for recording and measuring everything, "we have a social force that selectively encourages and rewards the scientific hack. . . . There is . . . a great hustle and bustle, a rushing back and forth to scientific conferences, a great plethora of $50,000 grants for $100 ideas. . . . I am suggesting that scientific, technical and financial facilities are such in this country as to encourage a great number of mediocrities to go into science and to seduce even those with creative talent and imagination to a mistaken view of the nature of the scientific enterprise."

When something lags in America, as basic research lags today, our temptation is to appropriate more money to it. But lack of money is not the root trouble in this area; $4 billion is not a niggardly sum. Fundamental research does not in its nature demand laboratories like palaces or an outpouring of heavy gold. An enormous amount of money is required to turn scientific knowledge and principles into concrete systems, and as a nation we constantly

confuse the two. The basic research that produced penicillin perhaps did not cost more than $20,000, but the developmental work to get penicillin into production on a mass scale cost millions. Our intellectual and social future depends greatly on our wisdom in finding where the small, crucial sums of money are to be spent for the nurture of truly gifted individuals; it does not depend on "crash programs" or the endless proliferation of equipment. But more than anything it depends upon the wise collaboration of government, industry, and the universities in this sphere, wherein they were joined by World War II and cannot now be put asunder.

Government's ambivalent role

Years ago, the thought of a federal government so deeply dedicated to the support of science might have seemed to scientists almost too good to be true. Today, the reality seems a little too true to be good. What objections are made within the scientific community itself to the government's support of science?

The strongest and most persistent objection is to the short shrift given basic research in the government's program. Many administrators of science feel that wherever funds are to be divided between basic and applied research, 10 per cent is the *minimum* share that should be allocated to basic. In the government program, basic research comes out with only 7 per cent; 93 per cent goes for research and development devoted to "hardware" for the armed services. (The Department of Defense spends almost three-quarters of the government's total research money; the AEC spends another 13 per cent; it takes only five other agencies to spend most of the small remaining balance.) World conditions probably demand that "defense" research have the funds allocated to it, but they do not demand that everything else be so neglected by comparison. The problem for science, moreover, is not essentially measurable in terms of money; the problem is the distribution of talented manpower (in acutely tight supply) that the proportion sets up. For

although we can appropriate dollars without limit, our resources of requisite brains for high scientific thought can be increased only by slow and careful nurture.

Intertwined with this objection to the government's research role is the bitterness most scientists feel at the government's excessive injections of secrecy and "security" into the world of research. Under the McMahon Act (liberalized only in 1954), nuclear technology, "civilian" as well as military, was almost totally isolated from the main currents of U.S. scientific thought, with results more injurious to America's technological progress than to Russia's. Under the McCarran Act (not yet liberalized), some of the greatest scientific minds of Western Europe became officially undesirable visitors to the U.S., to the humiliation, and impoverishment, of their American colleagues. The cumulative effect of these and similar events causes many a scientist to doubt whether he can continue to collaborate with his government on self-respecting terms.

"This Institution," said Vannevar Bush, in his 1953 report, speaking of the Carnegie Institution, which he heads, "has a magnificent opportunity in the future, *if national conditions are such that it can continue to prosper*." (Emphasis added.) Dr. Bush was by no means alluding only to the likes of Joe McCarthy; in common with many other scientists, he felt that a wave of anti-intellectualism was sweeping the country, with clear peril to the freedom of inquiry upon which science depends. He added that President Eisenhower's executive order on loyalty and security (the famous No. 10450) "was poorly conceived and poorly executed, and has made the situation much worse than it was before." In 1954, the handling of the Oppenheimer case threw scientists the world over into confusion and dismay. December of that year saw Warren Weaver, head of the Division of Natural Sciences of the Rockefeller Foundation and president of the American Association for the Advancement of Science, speak of "a present sickness in our country—a sickness of rumor and anxiety. Science is in a position to be particu-

larly aware of the dangers of this illness. But," he went on, "science asks for no privilege or protection. Science voices its concern primarily because the problem is a universal one." To this chorus the Committee on Research Policy of the American Council on Education added its voice: ". . . the American people must clearly understand that in this technological age the security and prosperity of the American family do not depend on a Maginot-line philosophy of rules and regulations, hastily drawn, often politically inspired, and born not of fact but of fright." But much Maginot-line philosophy still prevails.

And there are lesser difficulties. The governmental policy of support *via projects*, born of war necessity, and in contrast to support via gifted individuals, still continues and is a difficult habit to break. Means toward "avoiding excessive subsidy of the mediocre," in Dr. Bush's phrase, must constantly be sought. Huge government agencies like the AEC or the Department of Defense cannot take long shots in backing individuals, and the problem of getting various ponderous and conservative committees to give encouragement to what Dr. E. R. Piore of the Office of Naval Research calls "the young screwball" has so far proved insoluble.

But the coin has another side. The pattern of scientific administration in Washington, inherited from Dr. Bush in the days when he was head of the wartime OSRD, continues excellent. Two Washington agencies, both created postwar, typify the best in new approaches to government-sponsored research.

The ONR

The Office of Naval Research, by wide acclaim a model agency, was legally created by Congress only in 1946, but existed a year before that on the executive authority of Secretary of the Navy Forrestal. It is not, in the Washington phrase, an "exposed" agency; it reports direct to the Secretary of the Navy, and by its short but effective tradition, its Chief Scientist (i.e., executive scientific boss)

has remarkable autonomy in running his show. ONR can best be explained by the analogy that it is to the Navy what the famous General Electric Laboratory (below) is to G.E. With a 1955 appropriation of $60 million, it has some 360 projects under investigation, and farms its work out to some 217 universities and laboratories, in addition to maintaining a 4,500 staff of its own. It cannot devote itself wholly to basic science, but today, under the administration of Chief Scientist E. R. Piore (a civilian), its record for choice of research subjects, and the manner in which it maintains freedom on the part of researchers, make it, in the opinion of many, the model of a fine operating research agency—by government or any other standards.

The widely admired excellences of the Office of Naval Research do not stem from Dr. Piore alone; his line of predecessors was illustrious, too. Among them, the most immediate was a reticent but highly effective scientific administrator, Dr. Alan T. Waterman, who has now taken his gifts to that significant new institution, the National Science Foundation—the one wholly governmental agency in Washington whose devotion to basic science is complete. From this agency much is expected, and a fair amount, considering some difficulties with Congress, is already being accomplished.

The NSF

Long before World War II had ended, Vannevar Bush had been agitating for a new scientific focal point in Washington. His original name for the new agency was the "National Research Foundation," but before Congress finally created it as the National Science Foundation in 1950, the whole idea had been considerably mauled in the crosscurrents of postwar Washington. Dr. Bush's idea of an agency for fundamental-research guidance and administration had been all but buried under the different idea of an institution to subsidize inventions and "bring very practical matters into early use for the public benefit"—i.e., still more applied science, with

an additional touch of the mechanic arts. But when Congress finally did grant the NSF charter, for the basic purposes championed by Dr. Bush, it then forgot those purposes; having set a niggardly statutory limit on its annual funds of only $15 million, it appropriated only $3,500,000, and for several years NSF limped along with barely enough money to get itself organized into a small staff, and brood over what it might do if it had any money to do it with. "There is sorely needed," Dr. Bush wrote, a year after the founding, "an authoritative body where over-all policies [as to science] may be thrashed out for the guidance of the President and the Congress, and this the Foundation, under its charter from Congress, may supply."

Under its charter, and under the guidance of Dr. Waterman and a distinguished governing board, the foundation is now strenuously attempting to do just that. There is a certain inevitable amount of contradictory opinion among scientists today as to which of many possible functions the NSF should devote primary attention to. But at last the U.S. does have a prime scientific policy board at work in Washington, and when Dr. Waterman published his Fourth Annual Report, things were looking up for the National Science Foundation in a variety of ways. In 1954, NSF:

1. Disbursed $4 million in 375 grants for basic research in the natural sciences (compared to $2,750,000 in the previous *two* years).
2. Supported nearly 750 talented young scientists in graduate and postdoctoral study.
3. Put a special $2 million to work preparing for U.S. participation in the approaching (1957–58) "International Geophysical Year." (The I.G.Y. is a worldwide scientific program for global observation and examination of man's physical environment.)
4. Saw Congress lift all statutory limitations on NSF funds and give signs of reacting well to a request for $20 million for fiscal 1956. (Dr. Waterman believes the NSF could "profitably and wisely" spend from three to four times as much as this right now.)

It is Dr. Waterman's belief that the U.S. Government could "profitably and wisely" spend 50 per cent more than the $130 million of federal money that went to more or less basic research in 1954. This would increase the federal money to $200 million—still short of the 10 per cent of the total that many administrators feel to be the minimum ratio of basic-research support to total research support wherever applied, but certainly an improvement. And one of the most important, and difficult, studies NSF is at present making is a new evaluation of where and how the massive totals of research money are currently being laid out, and to what ends. The foundation is also examining the interests and scope of research in industry and the universities; as a result of this study we might at last be able to take bearings on the great voyage on which we have set out with so little idea of our destination.

Industry's role

"We in this country," said Alfred P. Sloan, chairman of General Motors, in January, 1955, "are not doing the basic research we ought to do in support of our applied research and our advanced engineering. We have got to expand our facilities for basic research."

When Mr. Sloan says "we" he means American industry. American industry has done an enormous lot of work it *calls* research, but so far it has neither supported fundamental science in any important way nor has it shown an understanding of the climate in which fruitful speculation can best flourish. Industry has been on an increasing research binge ever since World War II, but it still remains 99 per cent devoted to the linkages between applied research and the immediate needs of its sales departments, and its advertising usages have pounded the words *science* and *research* out of all shape in the public mind by invoking them in so many trivial and unworthy causes.

When Columbia University's Professor Rabi was asked recently

when he thought U.S. industry would begin wholeheartedly on the task of supporting basic research he answered with the two words "Not soon." But Mr. Sloan's statement may well be a prelude to a new recognition—so powerful is General Motors in the industrial world. His understanding of the basic-research deficiencies in America is only slightly defaced by his use of our ultra-American word *facilities*: the major facilities for basic research may be only pencils and paper, and a greater respect for Thought by society-at-large; these were Newton's "facilities," and Einstein's, and Planck's, to name only these. What today's American scientist needs, along with the money and facilities with which we are so generous, is—on the part of industry and the public—patience, understanding, and honor given to his status.

In their support of basic research, two corporations, but only two, stand in the front rank. These are General Electric and A.T. & T.—the latter as the supporter, along with Western Electric, of the world-famous Bell Telephone Laboratories. An indication of their stature is that each has produced a Nobel Prize winner: Irving Langmuir, of G.E., in chemistry (1932); C. J. Davisson, of Bell Labs, in physics (1937)—the only two Americans so far chosen from the ranks of industry-sponsored research. Next in rank behind these leaders stand du Pont and Eastman Kodak. Thereafter, an awareness of the importance of basic research is to be found flourishing in chemical companies like Union Carbide, Dow, and Monsanto; among petroleum refiners like Shell, Texas, and Standard of New Jersey; pharmaceutical houses like Merck, Lederle, and Upjohn; and businesses whose base is mathematical, like International Business Machines. But the huge automotive industry, with 1954 profits topping $1 billion, has yet to catch this drift; despite its enormous resources of money, apparatus, and gifted men, it clings still to the "proving ground" concept of research. There is nothing wrong with this except that, by itself, it is so insufficient. In contrast, consider the two leaders:

General Electric

The active support of fundamental research by this company goes all the way back to the beginning of the century. In 1900, it founded a laboratory intended for *applied* research. Electrical generation was then less than twenty years old, but the G.E. executive heads of that day were impressed by the debt their company's existence owed to the work of Faraday, Maxwell, Oersted, *et al.*, in Europe, and thought it only wise to see if a line of American inquiry could be perpetuated. Thanks to the good fortune by which they were able to bring Willis R. Whitney to Schenectady (from M.I.T., where he had been a young professor of chemistry), the laboratory not only flourished from the start, but shifted its focus more and more from applied to fundamental concerns. Since then, many of the most practical forward steps made in electrical utilization have come not from grubbing toward the mundane solutions of everyday problems, but by the G.E. policy of hiring the most brilliant men possible and leaving them to go their own gait. Today, so independent of practicality is much of the G.E. Lab's work that C. G. Suits, its present director, points to a fascinating reversal of ordinary cause-and-effect that often confronts him: "From basic research we achieve what appears to be an answer to a problem; *we then search through our complex technology for the problem.*"

The Bell Labs

Fundamental research in the Bell System got its start before the turn of the century with George Campbell, a physicist fresh from M.I.T., Harvard, and Europe. His monumental contribution to progress was the electric wave filter, which permitted many telephone messages to ride the same wires, and had even greater importance later in the coaxial cable and broad-band microwave transmission. About the time that Willis Whitney was leaving M.I.T. to join General Electric, Campbell brought to the Bell

System Frank B. Jewett, later the first president of the formally organized Bell Telephone Laboratories. The men Jewett later brought to join him formed an extraordinary community; they and Jewett formalized, organized, and promoted fundamental research for their industry, based on an interest in communications, the results yielding knowledge over an even broader range. Their basic philosophy continues to characterize Bell Labs today. The discovery of electron diffraction and the wave properties of electrons by Davisson and Germer brought Davisson the Nobel Prize in physics in 1937. Karl Jansky discovered the field now known as radio astronomy. Theoretician William Shockley had the insight to perceive that the junction transistor could and would exist, some time before others brought it into being with the help of Bell Labs' broad research program into the physics, chemistry, and metallurgy of semiconductors. The Bell Solar Battery also came from work in the same field, and may have opened a new pathway for the practical achievement of energy drawn direct from the sun. Claude Shannon, in 1948, published the now-famous Shannon-Wiener Communication Theory, which some scientists describe as the most important advance in knowledge since Planck's Quantum Theory. The list goes on and on. The invention of the transistor called for the refining of germanium to almost perfect purity; the Bell Labs staff responded with a process known as zone refining, which can be used to purify a wide variety of materials, not confined to metals.

The boss of this inspiring and inspired organization, Mervin J. Kelly, is himself an authority on thermionics and semiconductors; this year he must administer a budget of about $112 million for research. Of this, about $50 million will be on contract for the armed services. Work for the Bell System will probably divide into $35 million for Western Electric and $27 million for A.T. & T. And of this latter, which includes both research and fundamental design, about half would probably qualify as "uncommitted" research—

that is, research that will be undertaken primarily for the satisfaction of intellectual curiosity with no practical end in immediate view.

The universities' share

The first collaborations between government and the universities in the realm of science go back to the days of the Civil War and followed from the Morrill Act, which created the land-grant colleges: government gave, universities spent; there were *charters*, but no contracts. The date of the first collaborations between industry and the universities cannot be fixed, but they came later, and with some suspicion on both sides. So practical an institution as M.I.T. never until 1919 got around to accepting money direct from industry on any contractual basis. In that year it established its Department of Industrial Cooperation and Research, originally more as a gimmick to add funds to a post-World War I endowment drive than anything else—and was roundly abused even by some of its own alumni for this scheme of "selling Tech." But such a practice has long ceased to call forth apologies in educational circles, no matter how pure—and that the universities might be selling their souls for money from industry and from government by performing sponsored research has long since failed to disturb college presidents' sleep, except on the grounds that perhaps they do not charge enough for it.

The universities have not been forced to become subservient to government. But they are mightily dependent upon government. Nor have they become subservient to industry. But they are mightily dependent upon industry. As sanctuaries for the gifted, somewhat unsocial "young screwball," or the more elderly genius, they are not what they used to be. As Dr. Waterman of NSF points out, $4 out of every $5 of government funds in the colleges and universities "are provided for the applications of science rather than to basic research, and this is not healthy as the proper function of a university . . . The educational pattern also tends to respond by encouraging the premature study of the practical."

The university certainly remains the ideal locus for the larger part of fundamental research, but today's intermixture of fundamental and applied, and the side-by-side association of contract-supported research with the "undirected," produces a great many confusions in the aims and ends of scientific workers. Some scientific administrators think that applied science should leave the university atmosphere for good and that industry should perform as well as support such work. Some think just the opposite. Some point to the advantages of the so-called "Research Center," attached to a university, but separately administered and housed. This phenomenon, which had its beginnings under OSRD contracts during the war, is not free of its own problems, but it does at least segregate contract-supported research from any other. The university's role in research was confused by World War II, and has yet to be put right. So far as its relationship to government and industry is concerned, ONR's Dr. Piore has best summed up the problem and its solution: "The universities have got to stiffen their backbones as to the terms and conditions on which they accept money, both from industry and from government." After all, it is *their* science; it is *their* education. And it is *their* tradition of inquiry, and freedom, and pursuit of knowledge and truth. They must protect, defend, and expand it more aggressively than they do today.

As to foundations

The great foundations—Rockefeller, Carnegie, Ford—are today sources of perplexity to men in the administration of the physical sciences. Foundation interests have tended to veer away from the support of physical science in recent years—seemingly on the philosophy, curious to many, that if a thing is going well, it is time to drop it. The foundations' support of medical and biological research continues great, but it is the behavioral sciences that claim their increasing attention today. The physical scientist reacts to the foundations' present mood as Dr. Bush does here:

"The *project* idea [of research support], introduced largely . . . as a necessity at [war] time, is far better adapted to applied research than to fundamental research. This is part of the reason why fundamental research has not been expanded to the extent that it should be. The foundations here have to some extent missed an opportunity. As the government moved strongly into scientific research, they moved out. If they had moved into basic research they might have preserved a balance. But [they did not, and] in general the foundations have not tackled the problem of extending fundamental scientific research in this country, nor is there any great indication that they will do so."

This prophecy, made in 1953, still unfortunately holds good. But there are encouraging signs that a change may be on the way. The Alfred P. Sloan Foundation (originally for economic research) now has a new comprehensive program for the promotion of basic research in the physical sciences. For this purpose, $5 million will be added to the foundation's resources *as a preliminary fund*. Foundation grants will be made, in Mr. Sloan's words, "to scientists in universities and technological institutions who may be qualified to conduct investigations in various areas of the physical sciences that offer potential opportunity for basic scientific progress." This may be only a beginning; the Sloan Foundation's beneficence could be part of a new program for basic scientific support that might reach a total, for 1955, of $50 million to $60 million. At this level there might well come into being a kind of foundation, put into being by a pool of industrial contributions. Such a new creation would stand midway between industry and the universities, receiving from the one, giving to the other, but administratively beholden to neither.* The news is exciting, and the results could be impressive.

* *Smaller models for this invention already exist, e g, the American Petroleum Institute, which supports basic research in universities with funds (in 1954, $750,000) furnished by petroleum industry members; the University of Chicago Institutes of Basic Research, whose diverse industrial support about equals A.P.I.'s.*

A new day?

Between 1940 and 1955 the rush into research has been so pell-mell that there has been little opportunity for administrators to take thought. But the time for a new order is certainly now here. The National Science Foundation's forthcoming study of research may be one portent. The Sloan Foundation's plan is certainly another. Industry's own scope is broadening and, as one scientist put it, "a lot of businessmen are now convinced they'll really be in business a hundred years from now." Until very recently business and industry made contributions to "charitable causes" (education and research included in this awful definition) only on the basis of what the most cantankerous legal adversary could agree was "a legitimate use of the stockholders' money"—for state and federal laws made it hard for industry to rise above a charitarian's approach. The legal barrier has now been breached; much education of stockholders still remains. The number of industrial firms that *should* support basic research is not small; the number of absentees from enlightened research programs is still very large; besides the automotive industry, the mining industries and the smelters and fabricators of iron and steel and other metals have yet to see the light. Interposing a separately administered foundation between giver and recipient—to pool funds, to sort projects, and to find men—could launch a new era for basic research in America.

It is the last of these efforts that is always likely to be the hardest. No new foundation can subtract from the growing complexity of scientific administration. *Where is the man?* is the most fundamental question of all. Finding the genius, it is frequently pointed out, is the proper task for the super-genius. The late Daniel Coit Gilman, first president of Johns Hopkins, was such a super-genius, and no one quite comparable to him has ever arisen since his admittedly simpler day. No card-punch system can be devised to do the work he did, and the temptation to devise it must be resisted.

Future

Scientific circles emphasize heavily that we must take steps to improve the "climate" in which the finest and highest scientific speculation can flower, as it did in Europe before World War II. It is seldom suggested what these steps might be. Of the four native Americans who helped import the standards of European physics to America in the 1930's, two—Robert Oppenheimer and Edward U. Condon—have indeed resumed careers of scientific contemplation, but for the bitterly ironic reason that they are unacceptable by their government's "security" standards. The rise of the philosophy of "integration with the group" as the highest social gain points to no immediate improvement of climate for the fostering of "the young screwball" in physical science, but indeed quite the reverse. Meanwhile, the steady rise of scientific supergadgetry threatens originality from a different sector, less damaging than the political, but just as powerful in spite of itself. Any new foundations for the fostering of basic inquiry into Nature will have these twin handicaps to overcome, and money, by itself, will help with neither.

The mixed confession of shortcomings and faith that is our testament so far in this high domain was best set forth by Dr. Bush, in his Carnegie Report of 1953:

"The failure [to endow basic research] may be a reflection of our cultural immaturity. As we proceed, there may be men among us, highly successful in affairs, anxious to serve humanity, who will wish to look at the stars, or delve into the earth, or probe for the secret of life, not because it will add to the comforts or reduce the hazards of existence, but because it may render us a more dignified and understanding race with greater satisfaction in living. If so, there will be more and greater institutions devoted to the search for knowledge for its own sake."

April, 1955

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The Young Scientists

BY FRANCIS BELLO

If research in the United States is to thrive according to the prescription given in Chapter 1, then America's need for scientifically trained men has never been greater. What manner of man, then, is the scientist? Here is a recent portrait of the rising young men of science. It is men like these who are shaping our future.

IT MAY NEVER BE possible to measure the boundless energy and creative thought that a gifted young man can bring to bear on a problem that absorbs him deeply. This problem may be planning a military campaign or writing a sonnet, but the field that offers, almost by definition, the broadest range of objective problems is science. When the exceptional young individual runs head on into a crucial scientific problem, the results are often spectacular.

The recent history of Western culture strongly suggests that more and more of the finest minds have discovered in science a new frontier that provides an inexhaustible supply of problems of a uniquely stimulating and challenging character. This is not to make

invidious comparisons between science and other fields of endeavor, but simply to propose, as Thomas Huxley did in the last century, that "We are all scientists [because] the method of scientific investigation is nothing but the expression of the necessary mode of working of the human mind."

Huxley's remark should help to reassure many nonscientists who no longer see any resemblance between their mode of thought and that which seems to characterize the scientific frontier. Indeed, the loss of this resemblance may account for the gulf that many thoughtful scientists perceive to be growing between them and the rest of society—a cleavage that, if real, could have grave consequences.

Justifiably or not, many scientists view the 1954 security investigation of J. Robert Oppenheimer as a demonstration of this cleavage, indeed as part and parcel of an effort by some segments of society to pressure scientists into a narrow conformity that their whole tradition rejects. Conceivably scientists take a distorted view of this and other events, which they tend to interpret as evidences of anti-intellectualism. In any case, the Oppenheimer investigation may be of high byproduct value if it gives society some insight into the complex personality of the highly endowed individual.

While Oppenheimer's long letter replying to the charges made against him has been widely published, one of the key passages bears repeating: "I studied and read Sanskrit with Arthur Ryder," he wrote of his life in the 1930's. "I read very widely, but mostly classics, novels, plays, and poetry; and I read something of other parts of science. I was not interested in and did not read about economics or politics. I was almost wholly divorced from the contemporary scene.

"I never read a newspaper or a current magazine like *Time* or *Harper's*; I had no radio, no telephone . . . the first time I ever voted was in the presidential election of 1936." (Oppenheimer was then thirty-two.)

To be sure, it may never be the fate of another scientist to be so

centrally involved as Oppenheimer in a project of such surpassing magnitude. Nevertheless, the issues raised by Oppenheimer's portrait of the richly endowed scientist are broad and complex. To what degree, for example, may the scholar be excused from the ordinary duties of citizenship? Contrariwise, can society oblige him to be sophisticated in world affairs?

The scientist, particularly the most gifted, is, by almost any definition, a maverick. His endowments, drives, interests, political opinions, and even religious beliefs are not, in most cases, those of the majority of society. In the total population of the U.S. the scientist is at most 1 in 3,000, and if the term scientist is reserved for only the independent investigator, on whom virtually all scientific progress depends, the ratio is more like 1 in 30,000.*

How they feel about Oppenheimer

To learn something of the degree to which outstanding young scientists may differ from other people, *Fortune* interviewed twenty of the most highly regarded young scientists (under age forty) in the U.S.—ten from universities, ten from industry.† It also mailed a questionnaire to 104 outstanding young nonindustrial scientists who were part of the pool from which the ten academic scientists were selected. Of the 104, eighty-seven replied. Subsequently fifty of the group of 104 were resurveyed by wire for specific reactions

** Technically trained manpower in the U.S. totals about one million. Of this number only about 50,000, by virtue of a Ph.D. or equivalent degree, are ordinarily considered scientists No count exists, but perhaps only about 5,000 are actively engaged in independent research.*

† The ten from universities: Richard P. Feynman and Julian S. Schwinger (theoretical physicists); Andrew M Gleason and Walter Pitts (mathematicians); Joshua Lederberg and James D. Watson (biologists); Robert B. Wood-

to the Oppenheimer investigation, and of these, forty-three responded. From all these replies an approximate picture of the outstanding young scientist emerges:

► The scientists were referred to Robert Oppenheimer's statement that there was an extended period early in his career when he "never read a newspaper," and, in general, had scant knowledge of world affairs. They were asked what percentage of scientists today, in their opinion, were as uninformed as Oppenheimer formerly was. Their replies:

Doubt more than 1 or 2 per cent are like Oppenheimer	75%
Possibly 10 per cent are like him	22%
Surmise significant percentage are very much like him	3%

► Only a small minority defended the right of their colleagues to be poorly informed:

Scholarly training and status carry with them a responsibility to be particularly well informed on world affairs	74%
Dedication to chosen field may be all society can reasonably expect of the scientist or other scholar	26%

► As for the Oppenheimer investigation itself, a substantial percentage selected the option of making their own comment, rather than check one of the following:

Unpardonable attack on outstanding American	32%
Investigation justifiable in line with Administration's executive order of April, 1953	26%

Of the remaining 42 per cent, 13 per cent checked both of the above reactions, and 29 per cent stated a variety of individual positions. On balance, the great majority of scientists surveyed seemed deeply troubled by what they considered the clumsy han-

dling of the Oppenheimer case, even though all would undoubtedly concede the government's right to investigate anyone it wished.

Most of them are "eggheads"

When queried directly on their own politics, religious background, and current beliefs, the eighty-seven respondents displayed some striking patterns—not typical of U.S. society generally. For example:

► Eighty per cent of the scientists who responded voted for Stevenson in 1952. Broken down by scientific specialty, the Stevenson support ran as follows: psychologists, nine out of nine; biologists and medical researchers, twenty out of twenty-one; physicists and mathematicians, twenty-three out of twenty-seven; chemists, fourteen out of twenty; meteorologists, two out of two. Astronomers supported Eisenhower, five to three.

► From questions inquiring into the religion of parents and current religious beliefs, two striking statistics emerge: (1) the extraordinarily high percentage of scientists that come from Jewish homes; (2) a general loss of faith, regardless of religious background. The figures for the eighty-seven respondents appear below, together with latest figures for religious affiliation in the general population:

Religion or other	*Percentage distribution*		
	Parents' beliefs	*Current beliefs*	*Church affiliation of general pop.*
Catholic	5	0	19
Jewish	29	9	3
Protestant	53	23	34
Other	1	1	—
Religious, but no affiliation	4	22	—
Agnostic or atheistic	8	45	—

The percentage of scientists from Jewish families varied considerably in the several fields of science. In biology and medical research it was 52 per cent, in chemistry 35 per cent, mathematics 29 per cent, physics 18 per cent, psychology 17 per cent; and in astronomy and meteorology (the latter a tiny sample) zero. Among the twenty scientists interviewed, none was born (or is) a Catholic, and the proportion (30 per cent) from Jewish homes closely parallels that in the larger survey group. Among the same twenty the proportion who are now a-religious is considerably higher than in the survey group.

To account for these statistics one can only hypothesize. For example, the disproportionately high percentage of outstanding scientists with Jewish backgrounds might be explained by the scholarly tradition frequently observed among Jews. The absence of an equivalent scholarly tradition in a high percentage of American Catholic families might also explain, to a degree, the near absence of Catholic-born scientists in the survey group.

In the course of many interviews no woman under forty was ever mentioned as equal in ability to the outstanding men. Many graduate schools are reluctant to undertake the training of women because, as one scientist observes, "they have such a short half-life." Nevertheless, it is estimated that of 1954's 4,000 or so Ph.D.'s awarded in the sciences about 300 were conferred upon women, chiefly in biology, psychology, and chemistry.

Fun inside the nucleus

While the survey results suggest that the outstanding young scientist is markedly different from the average nonscientist, they say little or nothing about the gifted scientist's endowments or motives. It is in these areas that the scientist may differ most from the nonscientist. "We may never understand," says Warren Weaver, president of the American Association for the Advancement of

Science, "what it is that makes one individual consider it fun to labor for years on some esoteric scientific problem."

When scientists talk about their work, the word "fun" is never long absent. "People don't understand," says one of the brightest young physicists, "that scientists are just a bunch of guys trying to have the most fun they know how. If I knew anything more fun, *I'd be doing it.*"

The twenty young scientists interviewed by *Fortune* consider it literally fun to theorize about the nature of the atomic nucleus, or to study the structure of the universe, or examine the behavior of genes, synthesize complex organic compounds, study the age and origin of rocks, or theorize about the functioning of the brain itself. Few of them can recall when they first became interested in science, so early did their interest begin. At first it was not necessarily an interest in science per se, but simply a deep-seated curiosity about the physical world in general. The attitudes of parents ranged all the way from intensive cultivation of this curiosity to active opposition to a scientific career. Virtually all of the twenty enjoyed school immensely and stood high in their studies; a large majority were graduated at the top or near the top of their high-school classes. With few exceptions they read voraciously at home or in public libraries, were not very gregarious, and not particularly interested in sports. They absorbed knowledge so readily that the quality of their pre-college instruction seems not to have been very important. A few, however, report sympathetic and helpful high-school teachers.

Eighteen of the twenty were only children, only sons, or eldest sons. (According to normal expectancy only about ten should have been in these categories.) Only two—Julian Schwinger, a physicist, and Andrew Gleason, a mathematician—have older brothers. In her recent work, *The Making of a Scientist*, psychologist Anne Roe reports a similar preponderance of only children, only sons, and eldest sons among sixty-four eminent scientists (of all ages)

included in her study. It is her hypothesis that when brothers are just a few years apart in age they often compete so strenuously, with the younger losing out so often, that the younger may never acquire the self-confidence needed for success. This of course is *only* an hypothesis. And it may be noteworthy that in *Fortune*'s larger survey sample of 87 outstanding young scientists, there was *no* statistically significant predominance of only children, only sons, or eldest sons.

Public vs. private schools

In her book Dr. Roe reports of her sample: "By far the majority of these men went to public schools." This is hardly surprising, however, since only about one person in a hundred receives a non-Catholic private-school education. In striking contrast, 22 per cent of *Fortune*'s eighty-seven respondents went to private schools. The private-school graduates were distributed fairly evenly through all branches of science, but were fewest in biology (15 per cent) and most numerous in physics (32 per cent). Most of the private schools were not particularly well known. The three best known, each listed once, were: Phillips Academy at Andover, Phillips Exeter Academy, and The Hill School.*

Among the group of twenty, none of the academic men and only one of the industrial men ever attended a private preparatory school.

They're not farm boys

A study of physicists who are in *American Men of Science* supports a belief widely held in scientific circles that relatively small towns (under 2,500 population) have the best record for producing

* [illegible] of the leading private schools [illegible] *for approximately every 100 boys graduated in the period 1927–37. It would be surprising if this ratio has been equaled by many public high schools.*

outstanding scientists. This, however, was decidedly not true of *Fortune's* outstanding eighty-seven. Their distribution by size of boyhood community, compared to the population distribution of the 1930 census, is as follows:

	Percentage distribution	
Size of community	*87 scientists*	*1930 census*
Under 2,500	11	45
2,500–50,000	13	21
50,000–100,000	18	5
100,000–250,000	12	6
250,000–1,000,000	14	11
Over 1,000,000	32	12

Judging from these figures, communities under 2,500 were significantly unproductive of outstanding scientists. Communities between 50,000 and 1,000,000 did better than expected, and the very big city would appear to have provided the best environment of all. The last finding is explained in large part by the fact that 29 per cent of the survey group were Jewish-born and of these 65 per cent came from big cities—42 per cent from New York City alone. (Sixteen of all the respondents were born abroad: five in Canada, four in Germany, three in the United Kingdom, and four in Vienna alone.)

The depression of the Thirties may help to explain the poor showing of the rural communities. It was roughly in the period 1930–38 that most of the scientists in the survey were entering college. It may be conjectured that since the depression hit farmers particularly hard, fewer farm boys than usual had this opportunity. Contrariwise, in big cities it was common for high-school graduates, failing to get jobs, to enter a nearby publicly supported college or university.

The group of twenty closely follows the larger group in community background except that its distribution shows two sharp

peaks. Forty per cent of the twenty came from towns of 2,500 to 50,000 population; 30 per cent came from the big cities.

Any scientists tomorrow?

Those who fear that too few young people are entering science to meet current demands believe that the shortage can be traced, in part, to a decline in the quality of the teaching of mathematics and science in public high schools. While it is difficult to measure the degree of decline, it is widely believed to be true. What is certainly true is that there is a severe shortage of new teachers qualified to teach science and math. In 1953 there was an estimated demand for 10,000 science teachers; the supply was only 5,000.

In Russia, recruitment for scientific and technical training is pursued with a singleness of purpose unimaginable in a democracy. In a recent article on Soviet science in the New York *Times* Magazine, Eric Ashby, president of Queens University, Belfast, writes: "When he comes to make his choice [of a career], the Moscow boy is influenced by the immense prestige, deliberately built up by the state, of engineering and science. Newspaper headlines there go to scientists, not to the film stars or the football players or thugs. Every expedition to discover minerals, every new meteorological station in the Arctic, every improvement in making aluminum alloys, is written up in the racy style of a sports review."

Without the inducement of the immense prestige that Russia accords scientists, the U.S. may do well if it continues to reproduce in coming generations as many creative young scientists as it has today. Viewing the highly gregarious characteristics of the New Suburbia, as reported in *Fortune* and elsewhere, one wonders how many gifted youngsters will continue to find the solitude to read books and indulge their private curiosities. It will be interesting to survey the next generation of scientists, to see how many come from the Park Forests, the Levittowns, or from homes anywhere equipped with unsupervised television sets.

Where do they like to work?

Perhaps the most ubiquitous characteristic of outstanding young scientists is a fierce independence. This is invariably coupled with a strong desire to work on the most crucial problems in their field. As a consequence, industry, by and large, does not appeal to the most highly creative young scientists. Reason: industry, with few exceptions, makes its researchers stick pretty close to "practical" problems. In a speech to physicists, Thomas H. Johnson, director of research for the Atomic Energy Commission, has expressed with exceptional clarity the viewpoint of the creative scientist. He said: "The more specifically one tries to define the practical purpose of a research, the more indefinite becomes its bearing on the important questions of science and the less reason a scientist can find for doing it."

The two industrial organizations that have shown the clearest awareness of this viewpoint are General Electric and Bell Telephone Laboratories, and both have been, as a consequence, unusually successful in attracting top-flight young men. Their success was confirmed by the answers the survey respondents made to two questions. The first asked chemists, physicists, and biologists (scientists in other fields were exempted): "In your opinion do industrial laboratories contain any scientist in your field whom you consider equal in ability to the best scientists in universities?" Those answering yes were asked to name some of these outstanding industrial scientists.

Only 47 per cent answered yes, and their vote was divided as follows: physicists, 58 per cent; chemists, 50; biologists, 33. Over 65 per cent of all the physicists and chemists who responded had either once worked in industry or are now employed as consultants. This suggests that their answers (whether yes or no) were based on some firsthand knowledge. This was not so true of the biologists, of

whom only 24 per cent had worked for industry or are now consultants.

Who's who in industry

The vote is not very flattering to industry, particularly since most of those voting yes named comparatively few industrial laboratories where men of top university caliber might be found. Virtually all of the physicists voting yes named either Bell Labs or G.E., or both, and, all told, they listed six outstanding physicists in each organization. One Bell Labs physicist, William Shockley, who guided the basic research that culminated in the transistor, received eleven votes, and other Bell Labs and G.E. physicists received two or more votes.

The only other industrial laboratories that emerged with distinction were Lederle (a division of American Cyanamid), Merck, Kodak, International Business Machines, and Shell Development. These were the only companies in which the survey respondents nominated at least two different men as outstanding and gave one of them at least two votes.

Perhaps the most striking aspect of the vote was the poor showing of the big chemical companies. Three du Pont chemists received one vote each, and no chemists at all were named from such firms as Union Carbide, Dow, or Monsanto. The only chemists receiving more than a single vote were those employed by the companies (except I.B.M.) listed in the preceding paragraph—plus G.E.

How does it happen that the chemical industry, which spends more of its own money on research and development than any other industry, comes off so badly?

It is conceivable that du Pont and other big chemical firms have tried, without success, to locate eminent chemists. It seems more likely that the most gifted chemists are reluctant to work for the chemical industry. At the end of World War II, for example, one

bright young Manhattan Project chemist elected a university position that paid only one-fifth the salary he was offered by du Pont. The case of one of America's most gifted chemists is equally revealing. "I think I had an offer from du Pont," he says with a smile. "If it sounds odd for me to say I'm not sure, it's because I couldn't have been less interested." On the other hand, a brilliant young Harvard chemist recently accepted an offer from General Electric.

Science is a full-time job

Glaringly absent from the chemical industry is a willingness to give chemists as much freedom as Bell Labs and G.E. give to about 300 scientists. The chemical-company conception of freedom is well summed up by one major research director who said: "It is the policy of our laboratories, and of many others, to permit our men to have as much as 5 to 10 per cent of their time 'to go fishing,' i.e., working on anything that the man feels would be of interest, of course, keeping in mind the scope of the company's activities."

At least two of the big chemical companies give their first-string researchers 25 per cent of their time to go fishing. In one of these companies a research-group leader observes, evidently puzzled: "Hardly any of the men have taken advantage of the offer." The basic reason for the low response seems fairly obvious: to do something really worthwhile in science is a full-time job. And it is not easy to think of ways to attack significant fundamental problems. Those adept at this have not ordinarily chosen to work in the chemical industry in the first place.

Failing, evidently, to realize that young Ph.D.'s of deep curiosity and initiative seldom even apply to his company for jobs, unless forced to by unusual circumstances, the group leader mentioned above is again perplexed. "Practically all of our new Ph.D.'s want to be told what to do," he says. "They seem scared to death that we will ask them to think up problems of their own. The universities are wrong if they think we don't want and encourage new

ideas." By ideas it is all too clear that he is referring to ideas that will ring the cash register.

The say-nothing tradition

Much of the research success of Bell Labs and G.E. stems from the fact that they keep the cash register so discreetly in the background that no scientific sensibilities are alarmed. "The research laboratory has no responsibility whatever for keeping any particular operating department in business," says the head of one of G.E.'s research divisions. "But if the whole company goes bust, then it's our fault."

G.E. and Bell Labs scientists take it for granted that not only can they publish their findings freely, but they can also talk freely at scientific meetings. And as every scientist knows, good talk makes for good science. By contrast, most chemists from the big chemical companies are seen but not heard. Says one chemist: "They're nice enough fellows, they entertain freely, and they keep their ears open, but they don't communicate."

It is finally becoming clear to a few industrial chemists that the chemical industry's traditional policy of publishing little, and talking less, is handicapping their companies in recruiting a fair share of new graduates. "I've tried to tell top management," says a chemist with one of the biggest firms, "that we've got to begin publishing more to acquire a reputation in the universities. But I'm afraid it's a losing battle. They keep saying we've made all this money keeping our mouths shut, so why should we start talking now?" Such policies are reflected directly in the percentage of job offers accepted by new Ph.D.'s. One of the large G.E. research divisions has a recent record of 76 per cent acceptances. The record of one of the major chemical companies is 11 per cent. Thus, the rich get richer and the poor get poorer.

The wages of science

Bright young scientists who elect industrial research in an organization that honors freedom often discover advantages a university cannot offer. "It's easier to do research here than in a university," says one highly regarded G.E. physicist. "I don't have to go to the Office of Naval Research to try to get support, I have a well-equipped shop at my disposal, and I don't have to depend on students for help." A Bell Labs theoretical physicist says: "I can get a lot more work done here. I don't have to prepare lectures or discuss theses with graduate students, and the intellectual stimulation is just as great as at a university. Sometimes it's overstimulating." Both physicists have recently turned down offers to teach at major universities.

The lure of the university is powerful, however, and both G.E. and Bell Labs have lost a few good men in recent years. For example, John Bardeen, co-inventor of the transistor, recently accepted a professorship in physics at the University of Illinois. Says a G.E. division leader: "It's very hard to compete against an offer of a full professorship from a major university." In accepting a university offer, moreover, a young scientist does not necessarily make a financial sacrifice. If he is in his early thirties, the $10,000- to $15,000-a-year salary of a major professorship (e.g., at Harvard, M.I.T., or Princeton) may match or exceed what industry is willing to pay him at the same age. G.E., however, has quite a few scientists between thirty and thirty-five making $15,000 to $20,000 and a number under forty making $20,000 to $30,000.

The rule, of course, is that university salaries are significantly lower than those in industry. The median 1953 income (i.e., salary plus consulting fees) for all university scientists replying to *Fortune*'s questionnaire was $10,000. Medians by different fields were: physicists, $12,000; psychologists, $11,000; chemists, $10,000; biologists and medical researchers, $9,000; mathematicians, $8,500; as-

tronomers and meteorologists, $7,900. Except perhaps for the last category, the same scientists could expect to make about 25 per cent more in industry. (The two highest academic incomes reported in the survey were $20,000 and "$20,000-plus," for a chemist and a physicist, respectively.)

The survey's eighty-seven respondents also included ten scientists who work in government laboratories: five physicists and three chemists employed by the AEC (at Argonne, Brookhaven, Los Alamos, Oak Ridge) and two biochemists employed by the National Institutes of Health. These ten men have roughly the same average income as the academic medians for scientists in the same fields. The ten and a substantial number of their associates also enjoy as much research freedom as they would have in a major university.

While university salaries are often disgraceful, most scientists are uncomplaining. "A low income," says one biologist, "is the price we gladly pay for freedom."

Enough basic research?

It might be argued that industry's job is not to sponsor fundamental research but to get on with more practical matters. Actually, this is seldom industry's argument. Rather, research directors imply that they are encouraging just about all the fundamental work that can be justified.

The scientists in *Fortune's* survey, appraising industrial research from their academic vantage point, reach a different conclusion: 70 per cent of them say they believe "even the best of the industrial labs could justify significantly more fundamental research." Fifty-five per cent of the chemists took this view.

The real argument for introducing more fundamental research into an industrial laboratory is more subtle than simply hoping that basic work will pay off in a striking discovery like the transistor. Bell Labs, for example, believes that a new mating of basic and applied

research can lead to unsuspected and far-reaching insights that neither applied nor "ivory-tower" research would be likely to uncover separately.

Perhaps the most striking example of such an unsuspected insight is the Communication Theory published in 1948 by Claude Shannon of Bell Labs. While some of Shannon's work rested on the pure mathematical researches of Norbert Wiener of M.I.T., Wiener was apparently not concerned about putting even his preliminary work in a form anyone but a fellow mathematician could understand. As elaborated and elucidated by Shannon, the theory not only has proved of extraordinary value to communication engineers but has stimulated a vast amount of new thinking among linguists, biologists, neurologists, and psychologists. (See Chapter 15.)

"It is very puzzling," says one scientist, "why the basic-metals producers, the oil companies, and even the big chemical firms don't *do more fundamental research. Perhaps it's because they still see* themselves as essentially extractive industries. Bell Labs and G.E., on the other hand, don't have anything to sell but their ingenuity."

The "facts" of scientific life

When a young man picks a career he is normally guided by what interests him and what opportunities he sees in the fields he believes open to him. Seldom, however, is he presented with a dispassionate analysis of the long-term problems peculiar to various vocations.

Observing that many young men training for careers in science seem to be totally unprepared for the highly competitive and often poorly rewarded life ahead of them, a distinguished psychiatrist, Lawrence S. Kubie, recently addressed himself to what he called "Some Unsolved Problems of the Scientific Career." Kubie, clinical professor of psychiatry at the Yale School of Medicine, published his observations in two recent issues of the *American Scientist*. Since he presumably is not trying to frighten young men away

from a scientific career, but simply to expose them to some of the hard facts of scientific life, his observations make important reading for every prospective scientist.

His conclusions are sobering: "Certainly the idyllic picture of the innocent, childlike scientist who lives a life of simple, secure, peaceful, dignified contemplation has become an unreal fantasy. Instead, the emotional stresses of his career have increased to a point where only men of exceptional emotional maturity and stability can stand up to them for long, and remain clear-headed and generous-hearted under such psychologically unhygienic conditions."

How many breakdowns?

In reaching this conclusion Kubie refers to "the high incidence of 'nervous breakdowns' among scientists in their middle years"—an incidence that Kubie suspects may be significantly higher than in the general well-educated male population of corresponding age. In its wire survey *Fortune* referred to this statement and asked those queried to check one of three reactions, with the following outcome:

Just don't believe it	9%
Skeptical	60%
From my observations Kubie may be right	24%
No answer	7%

Concededly, the question is one to be settled not by vote, but by facts. Nevertheless, the minority vote of 24 per cent is too large to be dismissed lightly, particularly since one would assume that scientific caution should have made "skeptical" the overwhelming choice.

However, none of the scientists interviewed intimated that he would trade his career for any other. Said one chemist: "Kubie thinks we're all crazy because we work so hard. Maybe he's right, but who cares."

Probably Kubie's strongest point is that *in research, disappoint-*

ments are the rule, not the exception, and that a young scientist does not have many prime years to come through with a major scientific finding. Says one physicist: "Let's assume you get your Ph.D. at twenty-five. If you're going to do anything big you've almost got to do it in the first ten years or you probably won't make it at all. When you consider that a given problem may take two or three years of work, and that the chances of success are perhaps one in ten or less, you can see that the ten good years are quickly passed, and you may not have much to show for your efforts."

After drudgery, a thrill

It will do no harm, obviously, for the prospective scientist to appreciate this. On the other hand, outstanding success is the exception in any field of endeavor. What science—like mountain climbing—can offer is a broad choice of problems that vary tremendously in difficulty. The scientist may expect to find substantial satisfaction in solving any of them.

Indeed, what Kubie seems to overlook completely is the pure joy that many scientists report they experience on discovering a new effect or in solving a particularly knotty theoretical problem. On a small scale this thrill is known to everyone who has solved a particularly mean crossword puzzle or bid and made a grand slam.

"I don't know how other people get thrills in life," says one outstanding physicist, "but to me the biggest thrill is seeing a new effect for the first time. It may happen only once or twice a year, but it's worth all the drudgery that precedes it. It's like shoveling dirt in a gold field and suddenly turning up a nugget. When this happens, it spoils you and you'll never settle for less."

If there were any way to communicate this experience to young people, science would not have to worry any more about the supply of future scientists.

June, 1954

curiosity), the applied man's target is always visible. Little's contract researchers must accept more external discipline than pure scientists (the dollar-conscious customer is watching) and display more versatility than captive applied scientists (the target veers more). Also, their weightings of incentives—for example, money, opportunity to satisfy intellectual interests, security of tenure—are peculiar to them. In short, contract-research workers are a special breed of applied-science cat.

Some Little "miracles"

Little's 1954–55 clients have included such paladins of research as du Pont and General Electric. Some current clients are Armour & Co., Borg-Warner, Container Corp. of America, B. F. Goodrich, Lane Co. (cedar chests), Liggett & Myers, New Jersey Zinc, Owens-Illinois, Pittsburgh Plate Glass, R.C.A., Sears, Roebuck. All these have given repeat orders, Owens-Illinois eighty-two times, Lane annually since 1922. A sampling of some of Little's recent research "miracles" illustrates the kind of product that contract research delivers to its clients.

► A Little team led by chemical engineer Julian Avery developed the blast-furnace pressure top for Republic Steel, the first important improvement in blast-furnace technique since 1895. While working on this, Little's Dr. Bruce Old became acquainted with another rather static technique, that of the open hearth. Traditional methods of tapping furnaces were dangerous to personnel and to the quality of the product. In an earlier Navy research post, Dr. Old had overheard Dr. John von Neumann, now of the AEC, discussing shaped charges, used in the bazooka. The shaped-charge idea previously had been applied industrially only for safe cracking. Dr. Old had the idea of tapping an open hearth by a shaped charge. Little sold Republic a $16,000 research contract and developed a "jet tapper" that is now produced and marketed under a licensing deal by du Pont, a company familiar with explosives.

► In 1952, American Viscose decided to try to develop an idea for a fiber-forming spray device on which it held a patent. High-polymer experts were available in the company, but not the requisite mechanical-engineering talent. Since the company did not want such talent permanently, it approached Little. The latter offered a probability of successful research at a fee of $3,500 a month for twelve months. At his 1954 stockholders' meeting, Dr. Worth Wade, American Viscose's patent-development chief, made headlines: he had learned from Little how to spray rubber through a device, producing fibers that could then be collected into a web called Filastic, with unique qualities for use in girdles, inner soles, and other items.

► Bristol-Myers' brushmaking subsidiary, Rubberset, is an old Little client. Brushmakers used to depend on hog bristles from China as a raw material. In 1950, because of the Far Eastern situation, Rubberset asked Little to work jointly with its own laboratory to find a substitute. Little's team decided to seek a new, cheap source of keratin, a major protein component of bristles, and found one: chicken feathers.

► The Navy in 1935 asked Little how to make potable water in large quantities from sea water with equipment scaled to submarine use. Little began studying the problem at its own expense. Dr. Robert V. Kleinschmidt, a physicist and mechanical engineer who had studied the heat-pump principle, headed the research team. It developed the Kleinschmidt still. Some $100 million worth installed in the U.S. submarine fleet doubled cruising range in World War II.

► In 1927 glassmaking was largely an art. Owens-Illinois called in Little to help standardize and cut equipment costs. Little determined that viscosity range must be controlled, but before viscosity could be measured, Little had to develop a viscosimeter. With this tool, Owens-Illinois cut its forty formulas to six. Little also helped to develop glass fibers, the basis of the $140-million business of

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productivity, based in turn on the application of science to industry. "There is," Dr. Little preached tirelessly, "a thousand times more physical science waiting to be performed than has ever yet been attempted—and here we are to do it." Always, he meant, for a reasonable fee.

As research began arousing interest in industrial circles, tight-fisted Yankee corporate treasurers fought off proposals of expenditure on plant laboratories. Sometimes they conceded that Dr. Little might be retained, contracts to be terminable at the client's will. Starting in paper-production research, Little acquired a reputation and added new lines. In 1911, General Motors hired Little to show it how to set up a laboratory; thereafter, General Motors came to Little with special jobs. World War I gave the U.S. chemical industry and Little a great fillip by cutting off German suppliers. In 1917, the company splurged on its own home on Cambridge's Memorial Drive (Research Row's official name), then celebrated as "the palace of research."

For a generation U.S. contract research was dominated by Arthur D. Little, Inc. When, after forty-nine years as head of the organization, Dr. Little died in 1935, its character was a bit mixed. Dr. Little's earliest contracts had been not for research but for routine testing, and 17 per cent of the volume was in this byway in 1935. Testing, unlike applied science proper, has a noncyclical character, and Dr. Little never mustered the courage to throw a source of steady income out of the "palace of research."

Moreover, the company lacked an adequate sales tradition. True, Dr. Little once literally made a silk purse out of a sow's ear, just to show what applied science can do, but his promotion was quite sedate. He did not so much sell as accept orders. A few years after Dr. Little's death industrial research boomed all over the U.S., but Little vegetated in New England. The big demand in other regions was met by the setting up of nonprofit institutes. They began to perform a service in their areas analogous to that performed earlier

by Dr. Little in the older industrial areas of the country. Progressive industrialists, admiring the public-service aspects of such institutes, joined their boards and worked hard for them. During World War II government orders further swelled the business. Later, additional nonprofit institutes arose and grew rapidly. By 1941, Little had lost its volume leadership, never, so far, to regain it.

Groups and teams

But the intellectual leadership established by Dr. Little survives in the sense that his operating principles became standard for the trade: no assignment may be undertaken that is not scientifically justified; the client's money must not be wasted on superfluous equipment or otherwise; the client-researcher relationship is confidential (the client gets any resulting patent); no job is to be taken that creates a risk of one client's benefiting improperly from work done for another; contact with the client must be maintained primarily through a senior research worker on the contracted job rather than through a department head or officer; all employees must be available to contribute their special skills to help solve any client's problem; there must be a minimum of supervision, paper work, and red tape.

The administrative curses have, of course, increased with the multiplication of accounts and staff, but Little fights them off. Since 1954, the company has had five main divisions, each headed by a vice president, but being a division chief is not a full-time job. The president and all technically qualified officers are still active research workers. Administratively what matters is the "group," anywhere from four to twenty research workers of relatively homogeneous skills under the general supervision of a senior staff member. The senior, who has his own research to do, also supervises the members of his group administratively (assignments of work, salaries, etc.). Operationally, however, what matters is not the group

but the team. Teams, which may comprise as many as a score of scientists, often cut across group lines and division lines.

For example, the team that developed for the American Paper Goods Co. (now a Continental Can division) a novel paper cup, and then a machine to make it, included technical economists, market researchers, chemists (with specializations in plastics, paper, adhesives, and printing), biochemists (with specializations in toxicology, flavors, and odors), mechanical engineers, and machinists.

Today, counting only employees who work directly on clients' projects, Little has 184 chemists and chemical engineers, sixty-five mechanical engineers, thirty-six assorted engineers, twenty-seven physicists and mathematicians, fourteen biological scientists, and thirty-nine others (economists, designers, patent lawyers, etc.). On occasion Little retains specialized outside consultants. With M.I.T. just across the street—though that institution has sold the majority stock interest that Dr. Little willed to it—relations are intimate. Many M.I.T. faculty members are available for consultation over a telephone tie line.

The science director

Many members of Little's staff have been with the company for decades. And holding good men is vital to a research organization. The more types of problem that any given applied scientist works on the better. The man who, for example, led the team that solved Rubberset's bristle problem had earlier worked on nylon brushes, rubber cement, coffee concentrate, fishlines, rubber fibers, antifreeze, shrinkproof paper, hair wavers, cellophane tape, and other materials. Variety of experience has a value in itself, for unpredictable combinations may open the way to the solution of any given problem, and laboratory notebooks left behind are a poor substitute for the minds of departed staff members.

To get the most out of the range of skills available, Little must

combine and recombine them ingeniously. The biggest job of Little's management is not supervision but communication—effecting the maximum number of interconnections among skills to produce results fast. One device is the so-called "science director," who directs nothing, but consults, watches, questions. He is a physical chemist, Dr. Howard O. McMahon, formerly of M.I.T., a leading low-temperature expert, holder of several patents, and a winner of major awards for scientific work. Dr. McMahon does research for Little clients, but he also talks with team leaders to help make sure that they undertake only scientifically valid work, and achieve optimum use of manpower and exhaustive application of all valid techniques.

The typical Little job, then, is performed by a team of diversified talents operating almost as a temporary division of the client's organization. Charges—approximately double the professional man-hour salaries for any job—cover overhead and earnings. Duration and monthly budget are set by contract. Resulting patents are assignable to the client. The latter may terminate at will. The method and general setup resemble those that produced radar (much of it at M.I.T.) and the A-bomb, but they are older than World War II, and Little contributed importantly to their early development.

Beyond technology

In recent years Little has added new types of business and has adopted new methods of selling that have helped the company to grow. It has expanded its product line outside the physical-science fields. Since 1938, Little has served as technical consultant to Ferdinand Eberstadt's successful Chemical Fund. It has been retained by Merrill Lynch, Pierce, Fenner & Beane and many financial groups to do technical research for security issues. Little scientists systematically study clients' basic business problems (product selection,

process selection, plant location, raw-materials forecasting, and the like) from their special technical viewpoints.

In 1942, Little went into area-development work, undertaking studies for Puerto Rico's Operation Bootstrap. Today Puerto Rico has over 300 new productive enterprises, thanks in good part to Little's technical economics work. Little is now doing similar work in Jamaica, Canada, Egypt, and Iraq.

After the development of the scientific method of operations research in World War II, Little pioneered in applying this technique to business problems and was the first to sell it to a group of important clients. Little publishes a quarterly survey of the chemical industry and a similar institutional investors' service, for which technologists and economists pool their talents for trend spotting and forecasting. Subscribers include leading banks and insurance companies.

All this sort of work has brought Little into increasingly intimate touch with executives of client corporations other than the research directors who were the traditional point of contact. As a result, the company has had opportunities to undertake types of work familiar to management consultants and low in technical content. Little will not handle industrial relations; do accounting, job evaluation, incentive plan, or time studies; or consult on purchasing, credit-system, or office procedure. But it will undertake the definition of management authority, market research, and studies in general finances, costs, pricing, and distribution.

The trend toward business services of dilute technical nature is general in the contract-research trade and it looks good on income statements. Today one-quarter of all Little business is done by the Management and Business Services Division, and some top Little scientists are consequently uneasy. Contract research can easily get into business service areas that, however reputable and profitable, are not science.

Little has wandered into producing highly specialized hardware, the helium cryostat, and other elaborate, expensive research tools. Of these, $382,000 worth were sold profitably to research laboratories in 1954. Somewhat more diversionary than such operations is volume hardware production, thus far undertaken only for the government. That sort of operation can be more profitable in theory than in practice; after Little tooled and staffed for a production job connected with a classified project, the government changed plans and canceled its contract. Recently, to the relief of its top scientists, Little sold its half-interest in the Cambridge Corp., created to produce volume hardware, to its partner in that venture, Carrier Corp.

Scientists selling

Little's longhairs are enthusiastic about recent changes in selling methods. These are largely attributable to two officers. One, in charge of sales, is the senior vice president, Raymond Stevens. The other is the most junior vice president, D. Reid Weedon Jr. They alone among senior employees are businessmen rather than scientists.

Weedon recently sparked the setting up of Little branch offices. There are now offices in New York City, Chicago, and a laboratory in San Francisco. Stevens and Weedon are also behind a series of promotional luncheons being given in leading cities for batches of corporate research directors and other executives. Tables are decorated with samples of products that Little helped to create, the walls are hung with graphics, there are brief promotional talks and lantern slides. Little has a stack of new promotional leaflets, all more eye-catching than those of Dr. Little's time. All this may sound tame to salesmen of consumer durables, but it is a far cry from Dr. Little's decorum, and today every Little scientist acts as a salesman, helping to sew up contracts and keeping an eye open for new opportunities to sell services to clients.

Scientist managing

Ventures into new fields and intensified selling may embody some threat to the intellectual standards set by Arthur Dehon Little, but his successor is alert to counter any threat. Earl P. Stevenson, who joined Little in 1919, became president in 1935 when Dr. Little, shortly before his death, became chairman of the board. Stevenson holds a master's degree in science from M.I.T. and his scientific standing is high. He is on the National Science Foundation's board and is also chairman of the board of Connecticut's Wesleyan University. One of his major activities has been the series of Owens-Illinois and Owens-Corning jobs.

Stevenson upholds Dr. Little's best traditions and sometimes boosts them a notch higher. In 1937, he boldly dropped the remunerative but distracting business of routine testing. Some of Little's new ventures that have demonstrable scientific validity—atomic-energy research and operations research, for example—have been pushed by Stevenson. Some others have been undertaken despite his misgivings or indifference. Not all of these novelties are unsound. Stevenson, in short, is conservative.

Nothing concerns him more than to conserve the Little tradition of hiring and holding able scientists. To do that, Stevenson decided several years ago to equip Little with appropriate new devices. One is a bonus plan whereby, since 1951, 100 senior employees split up about 4 cents out of each dollar of company sales. Another is a profit-sharing system that feeds an employee-retirement trust set up in 1951. The result is a compensation level, immediate and deferred, that challenges that of captive research departments and surpasses that of nonprofit "institutes." Other attractions are Little's policy of encouraging employees to publish in technical journals and to participate in the life of science through scientific societies.

All this costs the company money, but personnel is what Little wants to spend its money on. One young institute has dogmatized

that "a research scientist is only as good as the tools with which he works;" but Stevenson prefers to spend money on brains that can get along with relatively simple equipment. Little's laboratories are full of makeshift apparatus with which its men do fine work. On the Viscose job they made their experimental spray device out of an old ball-point pen and some pipe fittings. When something fancy like an electron microscope ($18,000 per unit) is indispensable, Little can always rent instrument time.

Talent expansion

Such conservatism is in Dr. Little's tradition. And—as is indicated by the trade practice of publicizing intensively the names of distinguished recruits—it is talent that attracts and holds customers.

Through the addition of new talent, Stevenson hopes to expand into some fields that thus far have been the special preserves of not-for-profit competitors—metallurgy (Battelle Memorial Institute) and electronics (Stanford), for example. The following are typical of senior scientists recently recruited:

► Dr. Bernard Vonnegut, a physical chemist, who pioneered the seeding of clouds with silver iodide in General Electric's laboratory and whose basic interest is nucleation. Little gives him time for pure nucleation studies; meanwhile he gets into jobs involving nucleation phenomena. One is to develop methods of controlling the speed of plaster setting for National Gypsum Co.

► Dr. Charles J. Kensler, pharmacologist and biochemist, who left Cornell's medical faculty to join Little. He has held many fellowships and has published sixty scientific papers.

► Dr. Leslie G. Peck, who heads Little's computer laboratory. A former Johns Hopkins professor, he was associated with the Los Alamos computer program. Now he analyzes technical problems mathematically and prescribes for the computer needs of clients.

In elaborating Little's incentive system, Stevenson has had help

from a member of the board, a nephew of Dr. Little. He is Royal Little of Textron American, himself the authority for the statement that Arthur D. Little left his majority interest in the company to M.I.T. rather than to him in order to protect the company's scientific standards. While Roy Little has nothing to do with the management of the company, his counsel has been central in setting up the profit-sharing plan and the employees' retirement trust. It was he, moreover, who helped arrange, in 1953, the sale of M.I.T.'s Little shares to the trust, intensifying employees' incentives to increase profits. All these devices, of course, cut taxable earnings sharply, but they also limit available working capital. Roy Little had a hand in the solution of that problem in 1952, when the "palace of research" was sold to Harvard University and leased back. When added laboratory space was needed after the war, it was acquired in West Cambridge at Acorn Park (the acorn-oak maxim was Dr. Little's decorous way of summing up the value of research) but company policy is against investing in bricks and mortar.

Sharing the profits

Many scientists of nonprofit institutes leave to join captive industrial-research departments. Recently, for example, American Machine & Foundry Co. set itself up in engineering-mechanics research by hiring ten senior scientists who left one institute in a body. Little has less to boast of on this score, or it might be said that Little's formula for holding talent is superior. "Our compensation," says Stevenson, "aims to give all a sense of urgency and to persuade able men to make their careers with us." The main factor seems obvious. Conditions of work and atmosphere are generally comparable, but the institutes, operated "not for profit," may not legally share profits with employees.

The institutes use some of their untaxed net earnings as working capital, and that has helped them to grow fast. Another use is for

equipment, which most of them accumulate less conservatively than Little does. A third use is for some research in the public interest. Examples are area-resource surveys, compilations of specialized information made publicly available, and some pure-science work.

The institutes enjoy promotional advantages—publicity about equipment and public service and in some cases a link with an educational institution—but what counts promotionally above all else is reputation for quality of product. And this depends on talent. Today the institutes attract many first-class scientists, but some top consumers of research fear that, in the long run, the institutes will suffer in the competition for talent—unless, like some of Little's for-profit competitors, they try to match Little's new compensation standards.

Taxes in the trade

The government, however, may intervene so as to change the general prospect. For many years before 1950, the income-tax-law provisions that grant exemption to organizations doing exclusively scientific or educational work and distributing no profits were broadly interpreted. As late as 1951, a court even decided that Mueller Spaghetti Co.'s profits for the years 1947–50 were exempt from taxes because they were being paid out as dividends to New York University, the tax-exempt sole owner of the company. In 1950, Congress amended the law to limit tax exemptions of "unrelated business," and in 1953, the Treasury adopted a limiting regulation of its own. True, under the law unrelated business income is still tax-exempt when an organization is operated primarily to do "fundamental research, the results of which are freely available to the general public." But the regulation excludes from "fundamental research" that which is carried on "for the primary purpose of commercial or industrial application." Not much nongovernment business of the institutes is "freely available to the

general public," or is even fundamental in the sense of the regulation.

The trend of both Congress and the Internal Revenue Service toward taxing applied-research earnings is supported by a decision written by U.S. Supreme Court Justice Minton when on the circuit bench in Ohio. The case involved a testing laboratory that claimed to do exclusively scientific work. Judge Minton upheld the Internal Revenue Service in denying tax exemption, finding that the appellant's scientific methods were used not for science's sake but for the sake of business, as in almost every modern enterprise. The Supreme Court refused to hear an appeal. Later, income from "testing for the public safety" done by organizations that do not distribute earnings was exempted by an amendment to the law that is now being fought by the organized commercial testing laboratories. Were the courts to affirm the taxability of non-"fundamental" contract-research work, a new exempting amendment might be advocated by the institutes, but it might be opposed. The taxpaying research contractors have one spokesman in Dr. Stevenson of Little. He has publicly questioned the position of any contract-research institute that is paid to do a job in "the exclusive interests of a private corporation" if it does not pay taxes on resulting earnings, and has complained of sales promotion that emphasizes the not-for-profit feature.

If denied tax exemption in the future, the institutes could cut the proportion of profit-generating work done for individual industrial clients. The most experienced not-for-profit institute executive, Dr. Harold Vagtborg of Southwest Research Institute at San Antonio, has his eye on the law. He says that he seeks particularly projects that interest a whole region or industry. Such projects may be self-supporting. When not, and when untaxed surplus earnings are unavailable, they must depend largely on endowment income or gifts, and Southwest has been endowed by Tom Slick of Texas to do a job in his region's interest. Dr. Vagtborg suggests that South-

west has less reason to worry about a tax shift than do most other not-for-profit institutes.

Another course, in the event of a tax shift, might be for institutes to go on a straight commercial footing. Then competition for contract-research talent and for business might become keener than ever.

March, 1955

4.

The Long-range Planners

BY MORTON M. HUNT

Another major research setting is the purely theoretical laboratory. A paragon among these, and surprisingly analogous to the university laboratories, is the Systems Engineering Department of the Bell Telephone Laboratories, which works so far in the future that sometimes its engineers must plan on using equipment that has not yet been invented.

THE CONTEMPLATION of the future has long been a favorite pastime of poets and dreamers. For industrialists, however, it is a serious and workmanlike business. One of the most remarkable organized efforts to foresee the future is being made, day in and day out, by the long-range planners and the "systems planners" of the Bell Telephone System—a thousand or more specially oriented scientists and executives scattered strategically throughout its huge laboratories and affiliated companies.

These down-to-earth seers grapple with the future from nine to five daily, trying to outguess the vagaries of unwritten history. For

obvious reasons, successful prophecy is far more important in the communications business than in the diaper, auto, paint, or paper businesses. The telephone first made possible easy contact between separated people—and the sweeping impact of that commonplace facility is almost impossible to gauge. Hardly anyone realizes, for instance, that the telephone on his desk is but one set of controls in a single electric machine that lies spread across the 3 million square miles of the U.S.; his phone is physically linked with over 50 million other telephones by 185 million miles of wire and 200 million complex electric relays. An individual conversation by telephone is an act one takes for granted; but in 1953 alone Americans made 56 billion phone calls, and quite obviously a major part of the affairs of the nation could not have been conducted without the transmission of this ocean of talk.

The U.S., with only 6 per cent of the world's people, owns 59 per cent of the world's phones. Britain has only one-third as many phones per million, West Germany one-sixth as many, and Russia one-sixtieth as many. To what extent this superiority in telephone service is responsible for making the U.S. the world's richest and most productive nation, one can only speculate; yet since man is distinguished from the beasts principally by the facility with which he communicates, it must be that the telephone network has been vastly influential in making American civilization what it now is.

For such reasons, officials of the Bell System feel the weight of destiny on their shoulders, and pay well for the services of expert planners. The Bell System consists of the American Telephone & Telegraph Co., the headquarters organization; Western Electric, which makes and installs the equipment; Bell Telephone Laboratories, the research-and-developing outfit; and twenty-one "operating companies," which actually provide the telephone service. In each of these parts of the Bell System there are specialists in planning, from the businessmen of A.T. & T. to the scientific directors of Bell

Labs, to the managers of the local companies, who try to keep up with the growth of their cities but not outrun them.

In addition to all this, Bell Labs has in recent years set up an entire department whose sole function is to assess the probable needs of the future, and to make judicious speculations as to how those needs can best be met. Systems Engineering, as the department is called, consists of over 230 thoughtful, inquisitive engineers, physicists, and mathematicians who are occupied full time with the express duty of acting as scientific seers and architects-of-the-future.

The boss of these engineer-dreamers is a short, amiable, balding man named George Gilman. His planners are an assortment of scientists of all ages, shapes, and backgrounds, from old experienced telephone engineers to shiny-faced lads with fresh Ph.D.'s. All of them are clean of hand and shirt; they sit in offices, reading, arguing, doodling endless diagrams, and collecting masses of data, but seldom doing physical work. "We avoid inventing things," Gilman says. "It prejudices our neutral position."

"Anyhow," he adds with an Olympian wave of his hand, "we don't have to actually *build* a new mechanism to know that it is possible. When we need a new thing, we think out what it should do, talk over its hypothetical characteristics with the development engineers, and they build it for us."

Gilman's systems planners are scattered about the upper floors of the Bell Labs building on West Street, overlooking the New York waterfront. A future-predicter at work is nothing exciting to watch. He sits at a desk, reading technical reports and memoranda; he scratches his chin thoughtfully, tilts back in his chair, lights and relights a pipe monotonously, scribbles a few equations, and doodles a few diagrams. Sometimes he shuffles in and out of the great laboratory at Murray Hill, New Jersey, looking for new theories and inventions to help him, asking innumerable questions,

and nodding in unconvinced affability. He cannot talk without a pencil or a piece of chalk; he spoils hundreds of sheets of note paper, tablecloths, menus, and napkins with scribbled diagrams. He may perform a study known as "systems evaluation" to see how well some part of the Bell System is currently functioning, and he may go on to use the mathematical tools of operations analysis to disclose which factors can best be altered in the interests of greater efficiency; but systems planning—the architectural designing of the future—goes far beyond both of these.

Automatization, vintage 1910

Long-range planning is an old and honored policy in the Bell System, going back long before there was any special department entitled Systems Engineering. Originally the key planners were simply high-level engineers scattered about the Bell System in various jobs. Just about the shrewdest, yet simplest, piece of forecasting in telephone history was done some forty years ago. The subject in question was the use of operators versus some new and seemingly visionary devices for automatic (dialed) telephoning.

A few of the more farsighted telephone engineers began to wonder about the far future. (This was about 1910.) They looked at the curves of telephone growth; they dared to wonder if someday every family might have a phone; they pored over charts and worked on equations of probability. After a while, a few of these genteel radicals put down their pencils and agreed on one thing: "Unless we put in dialing by the customers themselves," they told the vice presidents, "the telephone system will someday collapse of its own growth. Within a generation you won't be able to hire enough girls to run the phone system even if you could get every eligible girl of the right age and education in the whole country." Within ten years automatic exchange equipment and dial phones were perfected and being installed throughout the country.

Since that time the number of phones in the U.S. has grown

sixfold; if no automatic switching equipment had been developed, the phone company today would need 1,500,000 full-time operators—and it currently has a hard time finding and keeping a mere 250,000 of them.

Equally remarkable foresight was displayed in a few small laboratory rooms at Bell Labs in 1926, when inventor Herbert Ives and several associates began to tinker around with whirling disks, spirals made of little lenses, and other improbable-looking mechanisms in a system called "television." In 1927 they invited a delegation of newsmen into the offices of Bell Labs and let them goggle at a flickery picture of Commerce Secretary Hoover, speaking from a brightly lit telephone booth down in Washington, his face and voice being sent to New York over telephone wires. Within the next three years they had also transmitted two-way telephone television, color television, and had even foreseen the future to the extent of transmitting a movie over a TV circuit from Philadelphia to New York.

None of this was for the purpose of getting the telephone company into the TV-producing business. (For that matter, nobody in Bell knew whether there would ever be a TV business, or how it could compete with movies and radio.) But the planning specialists of the Bell System had figured out that this thing logically had to become big in the future; when it did, the images and sound would have to be carried around the country via the telephone network, and it behooved them to find out how it would work, and what kinds of long-distance wires, amplifiers, filters, and the like would have to be built into future telephone installations to make that possible.

The TV system nobody knows

It is likely that many people believe (if they think about it at all) that the TV networks themselves developed, installed, and operate the coaxial cables and radio-relay towers that carry TV across the

country. The actual facts are quite different. Broadcasters have rented the long-distance facilities of the Bell System since the early days of radio, and TV has followed the same pattern. In almost all cases, when the picture and sound leave the control room of a TV studio, they travel downstairs in telephone-company wires, under city streets to a "TV operating center" run by telephone engineers, and are transmitted cross-country by Bell System radio relay or coaxial cable—both of which the Bell System owns, and which its own engineers designed over fifteen years ago, in response to the earnest pleading of the planners to make radio relay and coaxial cable capable of carrying, not only great new loads of phone conversations, but television as well. At the far end, other telephone engineers receive and reroute the impulses via underground lines into the local TV station's own control room, where the studio engineers finally take charge of shunting it into the local transmitter.

Neither radio relay nor coaxial cable, excellent as they are for carrying TV signals cross-country, is economically feasible for this local distribution of the signals within a city from the TV operating center to control room, or vice versa. The fine-gauge paper-covered wires that carry ordinary telephone conversations around a city could be pressed into service only by using expensive special repeaters (amplifiers), and even then 2 miles would be the practical limit because of introduced distortions. So back in the early 1940's the design engineers developed for this special purpose a husky shielded wire called a "video pair." Video pairs, far cheaper than coaxial, were still expensive; moreover, they would take up valuable space in telephone conduits that might be better used for ordinary telephone wires. Yet at a time when no huckster, producer, or business analyst was willing to bet his reputation on the future of TV, the systems planners of Bell Labs advised the directors of the telephone companies to start putting video pairs into the ground as part of every new telephone-cable installation when those cables ran past stadiums, theatres, or radio-studio buildings. Long before

TV was a paying proposition, the telephone companies had stuffed into the ground millions of dollars worth of video pairs—useless for anything but TV—in the firm belief that the special wires would be needed mighty soon.

All this sounds simpler than it really is. The systems engineers of A.T. & T.'s Operation and Engineering Department and the systems engineers of Bell Labs had to do more than merely conclude TV was a coming thing. They had to estimate what demands it would make on a national communications system, and put together a theoretical system—based on equipment that would become available in time—that could handle these demands, be compatible with the rest of the Bell System's business (which is, after all, basically one of handling telephone conversations), *and*, beyond that, be economically justifiable.

The biggest trouble with TV, from the engineer's point of view, is that it takes up too much space. Each channel occupies as much room on a radio-relay or coaxial-cable circuit as would serve 600 to 1,000 simultaneous long-distance telephone conversations. And *that* is a serious matter; for frequency space is a valuable and hard-won commodity.

"Television," the planners warned the management of A.T. & T. in the mid-Thirties, "now looks as though it would use up anywhere from 2 to 4 million cycles of bandwidth—the equivalent of 600 conversations. That much bandwidth can't be crammed onto ordinary wires." Management wanted to know what to do about it. The planners said that several researchers in the laboratories had an idea that although wide bands of frequencies would leak off a regular wire, they would stay on the inside surface of a tube with a wire down the middle of it. Other researchers, they added, had some notion that high-frequency radio waves, carrying a wide band of frequencies, could be focused like light and beamed from tower to tower. Either method would solve the problem of future telephone traffic—and of TV at the same time. After listening to these

alternatives the A.T. & T. management boldly spent an additional $10 million to perfect coaxial cable (the hollow tube) and nearly as much on radio relay, though neither of these systems would be needed for nearly ten years.

A special feature of systems planning known to most canny businessmen is the strategy of hedging one's bets. Lesser men than systems planners might, fifteen years ago, have occupied themselves with the question of *which* system—radio relay or coaxial cable—was the better one, and which should therefore receive the full force of future development. In the truly long-range view, however, both seemed excellent, lacking only the perfection of special unknown devices to make either one a whole workable system. So both were pursued; both were perfected; and today both operate together—compatibly—serving the same functions within the transcontinental telephone network. In soft, level soils, where a plow can speed along easily, cable goes in cheaper; in mountainous regions, radio-relay towers perched on ridge crests are a better solution. As for the future, the best minds at Bell Labs will venture no guess as to which system will eventually win out. Right now, 11,000 route miles of radio relay are strung across the country, and 9,500 miles of coaxial cable are buried beneath its soil.

Before color, a scrubbing job

Color TV raised a whole new complex of problems. The big *trick* in color is to crowd much more information into no more frequency space than that allotted to black-and-white TV. Ideally, black-and-white TV is allotted a bandwidth of about 4 megacycles; but in actual practice, especially when coaxial cable is used, it gets somewhat less. That does not matter much, since the upper third of the bandwidth carries information which doesn't affect the picture perceptibly. Even when the upper 1.3 megacycles are chopped off, the televiewer can barely perceive any difference in picture quality.

Color TV, however, has to be sent on 4 full megacycles, and even so, the only place where TV engineers can squeeze in additional signals to signify hue and intensity is in the upper part of the band, just where loss and distortion are most pronounced.

For this reason, a group of systems engineers worked out plans for new terminal equipment, repeaters, equalizers, and methods of maintenance to make the present coaxial-cable and radio-relay circuits capable of passing the color TV signal without distortion or loss. "We did a job of what you might call 'scrubbing up the circuits,'" says one of the group.

Frequency space: a limited resource

Such matters are the legitimate province of systems planners; but actually the finest grist for their intellectual mill consists of far larger issues, problems that exist over periods of decades. One of the largest is the matter of a vanishing natural resource—frequency space. Every message sent over radio occupies a certain range of frequencies, and the more traffic there is, the more difficult it is to fit everything in.

By modern techniques, it is possible to send and receive radio signals covering a range from about 100,000 cycles per second (the 3,000-meter wavelength) clear up to 30 billion cycles per second (the one-centimeter wavelength). That seems like room enough for all, especially in view of the fact that Morse code needs only about 100 cycles, and a phone conversation needs only about 4,000 cycles. Radio, however, needs up to 15,000, and TV about 4 million per station. As a result, the FCC has already divided up and parceled out the entire useful range of radio frequencies. There is practically no empty space left, except at extremely high frequencies that the engineers don't yet know how to use.

If the radio waves are so crowded, perhaps the better answer for the phone company is to expand along the lines of improved coaxial cables and wires. But this is no easy answer either; the wider the

band of frequencies put on wire or cable, the more amplifiers the engineers have to insert into the circuit, or the bigger they have to make the cable. That soon becomes cripplingly expensive.

In another generation, from past indications, the volume of telephone talk may easily double or triple, the number of mobile radiotelephones increase tenfold, the volume of transatlantic telephone business grow a dozenfold, and TV expand into fields only dimly foreseen. Such facts might give any systems planner pause. But having so paused and reflected, several of Gilman's men and others from A.T. & T. have worked out plans in which it appears possible that the enormous potential load (even including theatre network television) could nevertheless be shunted around the country by Bell System facilities, by modifying present cable, radio-relay, and switching systems. "We expect to be able to meet whatever load may arise," one of Gilman's assistants says, "and without lousing up the whole radio spectrum, either."

Waves beamed through pipes

One of the inventions that figure heavily in their long-range thinking about the frequency-space problem is called a "waveguide." Above 30 billion cycles per second, there is a great area of radio frequencies that aren't used. Unfortunately these frequencies begin to act a little like light—they are stopped not just by solid objects, but by clouds, smog, or even rain. Bell inventors started over twenty years ago designing hollow pipes in which the radio impulses could travel in their own atmosphere. Confined within their waveguide—which will be only a couple of inches in diameter—these waves will neither be affected by outside radio waves, nor affect any outside receivers. In contrast to older systems, which carry several dozen to several hundred phone conversations at one time, a single waveguide pipe could easily carry many thousands. One hitch, unfortunately, is that the copper pipe must be microscopically precise, both in manufacture and in installation; also, each will require a

fortune in terminal facilities to stack up and later unscramble the thousands of simultaneous messages. (Waveguides a few hundred feet long are actually in use nowadays on radio-relay towers, and cost about $12.50 per foot. No Bell System engineer will even hazard a guess as to the cost of a transcontinental waveguide.)

Phones in a jam

Another major concern of the systems planners is the customer—a cantankerous, ornery, and noncontrollable piece of the system. A small group of Gilman's men is continuously trying to analyze the ways in which customer habits affect the telephone network.

One of the things they worry about is the possibility that too many customers will choose the same moment to pick up their phones to place calls. Ordinarily the telephone system is geared to handle about one phone in twenty, at any given instant. If one phone in every ten were to be picked up at once, a serious jam could ensue. If one exchange were to be so jammed, the automatic equipment in other exchanges would hold its calls, waiting to get through, and so the jam might fan out from one exchange to another throughout parts of a city, or even a whole city.

This is no theoretical nightmare. During World War II a Washington, D.C., radio station announced free nylons to the first few callers; the resultant eruption of calls swamped one central exchange, backspread to others, and seriously snarled phone connections in the nation's capital and some adjoining points for more than two hours.

The job of the planners, of course, is to specify systems large enough, and with safeguards enough, to prevent this kind of thing in everyday use; on the other hand, for good economic reasons, they dare not overdesign the capacity of the system by any huge factor. In general, their planning is sufficiently adroit so that attacks of paralysis in the phone system have been extremely rare and short-lived.

But the big question is: what would happen if a major disaster or surprise bombing attack were to cause millions of people to make a frantic dash for the phone? The resulting snarl might temporarily immobilize all defense and rescue efforts. After considerable study, the planners suggested—and the operating companies have adopted—a safeguard to be used only in the gravest emergencies. In each major telephone exchange of the nation, an attendant by merely flipping several switches can temporarily cut off outgoing calls from some or all of the nonessential phones, allowing the civil defense setup, the military, the Red Cross, and other critical agencies to go about their business unhampered by fears of an overload. This emergency system has been tested in a few local situations, such as storm and flood, and has worked beautifully.

Officials of the operating telephone companies do the same kind of planning on their own, often without the assistance of Gilman's group. They have been planning the routes of new cables and radio [illegible]rs so as to avoid paralysis if any one city were wiped out. For instance, all national TV programs, all overseas telephone calls, and until recently all long-distance calls in and out of New York passed through one switching center in a building on lower Sixth Avenue. One good blast over that part of the city would have cut North and South apart and isolated us from Europe. Today a series of alternate exchanges in Newark and other points are handling about one-third of New York's long-distance business. Similar plans are being carried out in a dozen major cities.

An ash can full of plans

Among the sundry uncertainties with which Gilman's department has to contend, perhaps the greatest of all is the unpredictability of new discoveries and inventions. A few years ago, for example, several physicists at Murray Hill got interested in the odd properties of the metallic element germanium. When they got through investigating it, they had invented the transistor—a pea-

sized object that will do most of the things vacuum tubes do, last perhaps twenty times as long, use almost no power, and take up almost no space. As a result, several hundred pounds of plans, which had been worked out with excruciating care, and which dealt with such matters as the amplifiers, modulators, varistors, and such that boost your voice in loudness $10^{1,500}$ times (i.e., 1 followed by 1,500 zeros) as it crosses the country, may soon be ready for the ash can.

The transistor did little, however, to alter the larger outlines of the biggest and finest scheme ever concocted by the prophets of the telephone company. That scheme is called "FACD" (foreign-area customer dialing), which means long-distance dialing by the customer. In the full-fledged FACD system, a subscriber will pick up the phone, dial three digits plus the local number of any other subscriber in the country, and that's all. No fuss, no operators, no waiting; all America in his own backyard.

That idea, so simple and appealing, is actually the longest-range piece of planning ever undertaken by the Bell System. It dates back to 1933, when Dr. Frank B. Jewett, then president of Bell Laboratories, invited his associates to consider the problem of long-distance automatic dialing.

The heart of the problem was the nature of central-office switching equipment. The early automatic machinery for handling dial calls consisted of banks of fast-moving rotary switches and relays called "step-by-step" equipment. This equipment is logical, but unimaginative; it has to be told everything, including not only the destination but the best route. If it were to be used for long-distance dialing, a subscriber might have to dial a number like this: 057 076 097 157 2345. Even so, the machine could not automatically take any bypaths or alternates.

The planners concluded that a completely different type of central-office equipment was needed. They told the development engineers in broad terms what they wanted to do. "Make a system," they said, in effect, "that can accept the dialed numbers from

the customer, hold them in an electrical memory while it figures out the ultimate destination; look over all the routes from its own position to the destination; pick out and test the shortest or best one; if that one is busy, pick out the next shortest one or the next until it finds a free circuit; operate all the necessary switches; make sure it has the right number; and then disengage itself and get busy with someone else." As though that wasn't enough, they also wanted it to be able to call a human operator when the customer dialed an impossible number, wait for him if he forgot the last couple of digits and had to look them up, pull the plug on him if he took too long about it, and in general do everything in a judicious, intelligent manner.

The machinery that was finally perfected to fit this prescription is called "the crossbar system." The first toll crossbar installation went into operation in Philadelphia ten years after the engineers started trying to make it. A more recent version of crossbar required a patent application as big and heavy as a copy of *Gone With the Wind*.

Nothing happens overnight

Although FACD has been in the works for nearly a generation, it is emerging slowly because such a vast amount of expensive equipment in the U.S. was made and installed to do the local switching job before the planners had begun concocting their great plan. The cost of the change-over will run into many hundreds of millions of dollars. As a result, FACD is going to have to be born a finger at a time.

To some extent, it's born already. Formerly, anywhere from two to eight operators had to talk to each other to put through a long-distance call. Today, because of the installation of many crossbar exchanges and toll offices, 44 per cent of all long-distance calls are being dialed directly by the first operator the customer talks to. The next step will simply let the customers in those same areas

(where the operators now dial long-distance calls) do the dialing themselves. This ultimate achievement is no longer just a paper dream: it went into effect in the suburban community of Englewood, New Jersey, in November, 1951.

Since that time, the 10,000 telephone customers of Englewood have been able to dial directly any one of some 13 million telephone numbers in the U.S., covering large areas of the East, Midwest, and West. They dial the ten digits and wait about fifteen seconds * while the incredible nationwide machine makes its thousands of split-second decisions, tests its routes, double-checks its own handiwork, and then rings the other phone, anywhere up to 3,000 miles away. Another 10,000 customers just outside Detroit and 10,000 more outside Pittsburgh got FACD during 1953, and more communities will get it each year from now on. The job should be completed within fifteen or twenty years.

Who makes out the bills?

The FACD plan solved many problems, but raised new ones. Who, for instance, would record and charge the customer for the long-distance call, if no operator were involved? The planners foresaw this need and predicted long ago that an automatic billing mechanism would have to be developed. Ten years of work in Bell Labs have since resulted in AMA (automatic message accounting). On a wide tape, AMA machinery records your phone number, the number you dialed, and the beginning and ending time of your conversation. Later other machines rerun the tape, pick out your call from all the other calls recorded on it, figure out how much to charge you, and write out a charge slip.

And how, the planners wondered, could a wholly automatic long-distance system be guarded from mechanical failure, with no operators checking on each call? (Actually, there will be even more

* *In 1920 the average long-distance call was put through in fourteen minutes; in 1953 it went through in ninety seconds.*

employees than there are today, because business will be so much greater.) The solution lay in giving the crossbar machine and the big automatic toll centers the ability to recognize when something goes wrong in their intricate mechanisms. When any part of their many circuits goes wrong, certain automatic checking devices fail to get the right response to coded testing signals they continually send out. The faulty part causes the automatic testing brain to punch a mark on the appropriate part of a long ticket, which graphically portrays in terms of preprinted code symbols thousands of possible trouble conditions.

Robot repairmen?

The obvious next step—and one that would surprise no one in the planning department—would be the development of servomechanisms that will analyze the printed trouble report and make simple repairs automatically, plugging in spare relays and tubes, pending the semiannual visit of the repairman.

All this sounds like plenty to work on for years to come. But in their more expansive moments around the conference table, the planners talk about even bigger things. Their FACD plans already make room for Canada and Mexico in the ten-digit dialing setup. But that's not all. "You may think worldwide dialing sounds silly," says one switching expert, "but we've thought about it a good bit, and aside from the backward state of telephone systems in some countries, it's well within the framework of our present plans."

Another remote subject of planning efforts involves a system that might, for the lack of any other name, be called "televisiphone"—the sending of TV images along with the voice signal. Bell engineers first hooked up a two-way telephone-and-television combination in 1930. The picture was miserable, and the cost would have been staggering to any customer. But it was a fascinating idea.

Today, twenty-four years later and with TV a nearly perfected art, the Bell planners are still thinking hard about televisiphone. It

would put a terrific new demand for frequency bandwidth on the telephone wires and undoubtedly cost a good deal. One idea the planners have recently been considering involves sending not a moving image but a series of stills at five second intervals; this would use up only a narrow band of frequencies and be much cheaper.

Dr. Ralph Brown, vice president of Bell Labs, feels that the use of vision on the phone is as little appreciated today as the use of speech was when Bell invented the thing three quarters of a century ago. "People used to ask who'd want to talk into a tin box," he says. "Today they can hardly get along without it, but they ask who needs TV with his telephone. But in today's world, sight and sound go together. Some form of vision with the phone is inevitable."

Other new devices that already exist, or should exist in the future, and for which the planners have great hopes, include an automatic telephone-answering machine to take your calls for you when you're out; a machine that understands the numbers zero to nine, and can ring a phone number upon spoken command; an electronic calculator that will design and draw plans for new pieces of technical equipment when told what that equipment has to do, and another calculator that will be able to translate from one language to another.

"However all that may be," Gilman said recently, staring at the ceiling with his hands clasped behind his head, "we can't afford to deal in idle dreams. We're simply trying to use every reasonable idea and prospective invention that may fit into the broad picture, so as to help solve the problems that will be arising in the future.

"The main thing is not to get too smug."

May, 1954

5

The Inventor in Eclipse

BY EDMUND L. VAN DEUSEN

Often veiled or forgotten among the industrial laboratories, the independent research companies, and the theoretical planners, are the lone and independent inventors. They shrink in number annually, and over the past quarter-century the number of patent applications in proportion to the population has declined 40 per cent. Yet historically it has been the independent inventor that has given technology its biggest boosts.

WHEN PERCY C. SPENCER, president of Sinclair Oil Corp., inaugurated the "Sinclair Plan" in 1951, he thought he had found a way to spice industrial technology with the offbeat yet valuable inspirations of independent inventors. Spencer invited "inventive-minded Americans everywhere" to send their ideas to Harvey, Illinois, where the staff of Sinclair's new $10-million research laboratory would evaluate the schemes and complete the development work on those that looked promising. All Sinclair wanted in return was a royalty-

free license under the inventor's patent—and a sense of industrial accomplishment.

Three years later, Sinclair had obtained exactly nothing from its plan. Fewer than fifty of the thousands of inquiries and suggestions pertained to the specified subject (petroleum products), and of these only three seemed worth extensive testing. Two proved impractical, and the third, involving a new way to make cement, was returned for commercial development to the original inventor, a Cleveland consulting engineer. Sinclair's conclusion: "There appears to be little room for the independent inventor in the petroleum industry."

The failure of the Sinclair Plan is of deep significance to American industry. In an era of unmatched technological progress, the lone, inspired inventor seems to be playing a diminishing part. At the same time, the rate of American invention seems also to have declined. In 1920 the U.S. Patent Office received 7.7 patent applications for every 10,000 citizens; today, despite a slight upturn in the trend, the rate is 4.6, a drop of about 40 per cent.

This does not mean, however, that the inventive Yankee is a vanishing breed. Anyone willing to set himself up as a recipient of ideas can attest to the contrary. During World War II over 300,000 suggestions were submitted to the National Inventors Council, an agency created at the prompting of Lawrence Langner, the eminent patent attorney (and founder of the Theatre Guild), to be a clearinghouse for new devices that might be of value to the armed forces. One such device, an instrument invented by a treasure-hunting Florida radio mechanic, turned up on scores of beachheads as the familiar mine detector.

After the war the council's experience was duplicated by the Institute of Inventive Research of San Antonio, a nonprofit organization put together by Thomas Slick to provide a connecting link between inventors and industry. In eight years the institute managed to evaluate over 100,000 inventions submitted by the

public, and from this group was culled the lift-slab method of concrete construction (used in building the $5-million laboratories of the Radio Corp. of America). But the costs of development of this and a dozen other promising ideas were so high that the institute, after running through its $1-million endowment, closed its doors to new inventions and became a part of Slick's Southwest Research Foundation.

Where Slick left off, however, the Product Development Corp. of Boston has taken over, and on a profit-making basis. Product Development is a year-old affiliate of American Research & Development Corp. (Reaction Motors, Tracerlab, etc.), and inventors are already sending their ideas to this new company at the rate of fifty a day.

Certainly there is no shortage of inventors and inventions, but then, that was not the problem encountered in the Sinclair experiment. The trouble has been quality, not quantity, and the reason for this is easy to see. For several decades, the important frontiers of technology have been advancing into the far reaches of chemistry, electronics, and, of late, nucleonics. Independent inventors, inadequately trained and equipped, can hardly hope to follow. The result is that these areas have become the preserve of the hired inventors who work in corporation laboratories. The lone, unaided inventor can do little but concentrate on the mechanical-type inventions, the better mousetraps, that were the marvel of the nineteenth century but today are considered to be only gadgets.

Invention is whose business?

Industry is keenly aware that the present quality of independent invention is low (General Electric finds that it is interested in less than one-tenth of 1 per cent of the 2,500 ideas and suggestions submitted to it each year). Yet industry also seems to assume that this low quality is none of its business (G.E.'s patent department has three men screening the public's ideas, seventy men processing the

3,000 to 4,000 patent suggestions developed annually by the company's own research laboratories).

Actually, industry's indifference toward the lone inventor and his works is understandable. Modern industrial research, with its fine laboratories, its teams of specialists, and its multimillion-dollar budgets, appears able to provide all the technological progress that the nation could desire. Indeed, amid this array of inventive power even such an obstinately independent inventor as Thomas Edison would find it hard to locate neglected areas in which to apply his genius. The easy assumption for industry to make is that the best inventors of this generation have all read the signs and are busily inventing within the walls of industry's research laboratories where, in the words of one director of research, "no one ever makes a million, but then, neither does anyone starve." From this, it is logical to assume that virtually all the important inventions of the future will come out of the major laboratories.

This reasoning, however, is not so airtight as it seems. The history of invention indicates that truly significant innovations generally are the work of "outsiders," individuals far enough removed from an industry to have a fresh viewpoint on its problems. Janney, inventor of the automatic railroad coupler, was a farmer, not a railroader. Cartwright, inventor of the power loom, was a minister. Eastman, a bookkeeper, revolutionized photography, but it took two musicians, Mannes and Godowsky, to show Eastman how to make photographs in color. Even television, a "modern" invention, would have been impossible without the inspired tinkering of generations of amateur inventors. Edward T. Dickinson, executive assistant to the president of Carrier Corp., conducted a study of this sort of intuitive innovation while vice chairman of the National Security Resources Board (the project was prompted by the belief that a creative atmosphere is an important "natural resource"). Dickinson concluded that such "leaps into the blue," whether by inventive individuals or by a whole population developing a new

civilization, occur at the time when those people have become isolated or withdrawn (e.g., the colonial period in America). For lone inventors, such withdrawal comes naturally, but it is extremely difficult to withdraw from one of industry's organized "teams" of researchers.

The independent "doodle"

Most industrial laboratories are also handicapped because "they think they can't afford to doodle," says Benjamin Franklin Miessner, a successful independent inventor who made good in 1930 by selling fifty radio-circuit patents to R.C.A. for $750,000. Miessner, still vigorously inventing, has great hopes for his latest doodles, a stringless electronic piano and a photoelectric phonograph, neither of which represents the type of project a corporate research director would choose to put in his annual budget.

Miessner's point is aptly illustrated by the inventive career of Edwin H. Land, inventor of Polaroid film and the Land ("one-minute") camera. In 1932 the total market for a cheap source of polarized light (used, at that time, only by experimenting physicists) would not have supported even the most modest of industrial-research programs. It fell, instead, to Land, an inquisitive college boy (Harvard), to conceive and develop a method of orienting tiny crystals in a sheet of plastic. Polarizing sunglasses, camera filters, and 3-D movies came much later—and took Land out of the class of lone inventors. By 1945 he was an industrialist, the president of his own successful company, Polaroid Corp., and director of a large research organization. And when he decided to "invent" a one-minute camera, Land proceeded in a typically industrial way. The camera was chosen because it was close to Polaroid's principal field (optics), yet different enough to provide good diversification for the company (no polarization is involved). Market surveys determined the weight, size, and cost limitations that the new camera had to meet. The self-developing character-

istic was included because Land knew he could not break into the camera field without an exclusive feature. It took three years for a coordinated team of chemists, opticians, and mechanical engineers to produce the one-minute camera exactly as Land had "invented" it.

The Land camera was an inventive tour de force and a great commercial success. But the camera will never rank as a "great" invention, i.e., one that opens up new avenues of research or helps to build a new industry. For that type of accomplishment, Land must still point to his youthful, independent "doodle"—polarizing film.

The lost incentive

While the long advance of technology has largely contributed to the independent inventor's low estate, it is hardly the complete explanation. Charles Goodyear spent eight years—and landed in debtors' prison three times—searching for a better way to cure rubber. With only a fraction of that energy and sacrifice, modern inventors could gain proficiency in almost any area of technology they chose. (Expensive equipment is often needed to prove an idea —but not to conceive it.) Few inventors, however, take the time or trouble. Foorman Mueller, a Chicago patent attorney and past president of the American Patent Law Association, observes that "in the past twenty-five years there has been quite a change in the individual inventor; there is less initiative to carry a project beyond the idea stage, especially in that percentage of the population which in the early days would spend days, weeks, and months, untold energy, and their last penny to bring an inventive dream to fruition."

Mueller, as a patent lawyer, is in a particularly good position to diagnose the cause of it all, for it is the steady weakening of the patent protection that is helping to sap the initiative of the independent inventor just when the complexities of technology are

putting an extra strain on his tenacity. The patent system is the independent inventor's traditional incentive system, and when the value of patents drops, so does the drive to invent.

Federal statutes lay the ground rules under which the patent system operates, but these laws are constantly being interpreted by Patent Office examiners, by federal-court judges, and by plain businessmen everywhere. And it is these individuals who in the end determine a patent's value. The Patent Office must decide correctly whether the invention is truly "new." The courts, in turn, must decide, in the case of a patent in litigation, whether the Patent Office decision was justified (if they think not, they can invalidate the patent at any time). Finally, the business community must decide, after studying what the Patent Office and the courts have been doing with similar inventions, whether it is safe to invest in the new patents being offered for exploitation.

To hit the jackpot on his invention, the inventor must win all three decisions; it is not enough for him to gain a solid patent from the Patent Office—one that infringers would find it very difficult to invalidate through court action—if the "market" for patents is unsettled because of uncertainty over the standards by which the office and the courts are operating. Such uncertainty now prevails.

Judicial confusion

The uncertain value of patents is largely blamed on the Supreme Court itself, although that august body passes on only two or three cases involving patents a year. Yet the decisions of the court have been a marvel of consistency; only five times in fifteen years has it judged in favor of the patent holders, a record that finally led the late Justice Jackson to predict, in a dissenting opinion, that the day was coming when "the only patent that is valid is one which this court has not been able to get its hands on."

Even more important than the decisions has been the language used by the Justices in their invalidating opinions. Justice Douglas, in a case involving an automobile cigarette lighter, introduced the

"flash of genius" test by writing that a new device, "however useful it may be, must reveal the flash of creative genius, not merely the skill of the calling." And in the famous A & P case, involving a supermarket checkout counter, Douglas again set a prohibitive standard by insisting, in a concurring opinion written with Justice Black, that "the invention, to justify a patent, had to serve the ends of science—to push back the frontiers of chemistry, physics, and the like; to make a distinctive contribution to scientific knowledge."

Influence of the Court

The Supreme Court decisions have had a profound effect on the lower federal courts. The most extreme example is the Appeals Court of the Second Circuit, which includes the important New York district court where one-fifth of all the patent cases in the country are initially tried. From 1948 through 1953 the Appeals Court heard cases involving thirty-eight patents; in thirty-seven instances, the patent was declared either invalid or not infringed; in only one case was the finding in favor of the patent holder. Such judicial precedents are not lost on the bargaining agents in a negotiation between an independent inventor and the business interests he is trying to entice.

In 1952 an effort was made to counteract the court decisions by incorporating a more moderate "standard of invention" into the new patent-law code adopted by Congress that year, but the few courts that have commented on the matter have maintained that the intent of Congress was primarily to codify, and not to modify, the existing patent laws. Another proposal, made by the Patent Equity Association of New York, is to create a system of technically trained judges for the hearing of patent cases. Such judges, drawn primarily from the ranks of patent lawyers, would preside in all patent cases.* But most patent authorities are agreed that little can

* *The present Court of Customs and Patent Appeals in Washington, D.C., is, like the tax courts, organized to adjudicate between the government (i.e., the*

be done until death or retirement changes the character of the courts; too many of the present judges were appointed during the era when Thurman Arnold was concentrating his antitrust fire on the beneficial monopoly of patents.

Jeopardized quality

Meanwhile the value of patents is being eroded by trouble from a new direction. Indifference and neglect are reducing the effectiveness of the Patent Office itself, and this will eventually have a direct effect on the quality of the patents that the office issues. A patent may be invalidated at any time if someone can prove to the courts that the invention was not "new" when it was applied for. It is the patent examiner's job to look for "prior art," but the Patent Office, unlike a title company, makes no guarantee that this search was complete. The inventor and his backers must proceed with commercial development at their own risk.

It is the quality of the Patent Office search that is jeopardized. Each year an additional 40,000 U.S. patents are added to the prior art, along with an equally large number of foreign patents—and tons of technical literature in every language. This, plus the increasing complexity of the individual patent applications, has doubled, in the past fifty years, the time and effort needed to process an application. Worse still, the 650 examiners are so rushed in their routine work (they are 200,000 applications behind) that there is no time to do the classifying and collating necessary to make sense out of this mountain of information. And now, instead of giving the Patent Office the help it needs, the present Administration has chosen to put the office on a stringent economy regime. The bureau's budget for fiscal 1955 was $11,500,000, down a half million

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Patent Office) and the public. Most patent cases of importance, however, are between two citizens and generally involve a patent infringement. The government, in issuing a patent, grants a monopoly for seventeen years, but the patent holder must defend that privilege on his own.

from the $12 million that it has spent in recent years. To make the office more nearly self-sufficient, the Department of Commerce (parent to the Patent Office) has asked Congress to raise the basic fees for obtaining patents from $60 to a sliding scale that approximates $120 per patent. Yet even this increase will cover only half of the current $6-million deficit.

These efforts at economy come at an inopportune moment. The Commissioner of Patents, Robert C. Watson, a respected Washington attorney appointed by the Republican Administration, calculates that the Patent Office needs immediately an additional 200 examiners just to stay even with the incoming work and to effect a small annual reduction (5 per cent) in the backlog of waiting applications. This reduction is particularly important because it is the application backlog that determines the length of time, now an average of three years and seven months, between the date of application and the issuance of a patent.

An Advisory Committee on Application of Machines to Patent Office Operations has been formed under the chairmanship of Vannevar Bush to study the possibility of mechanizing at least part (possibly 20 per cent) of the examiner's work. But this is long term, and of immediate moment is Watson's need for a special fund, possibly $5 million or $6 million, spread over as many years, with which to tackle the growing job of reclassification. (Searches are made by "classes" of inventions, and some of these groupings have not been brought up to date in years.) Unless there is some radical change in the Administration's thinking, however, Watson probably will not get even this short-term aid. And the value of patents will continue to diminish.

Industrial responsibility

The independent inventors, as a group, are too disorganized to do anything concrete in their own defense. This raises the question of whether it is industry's responsibility to use its weight and in-

fluence on the independent inventor's behalf. There are two good reasons why industry should. First, there is a strong presumption that industrial, organized research cannot yet entirely supplant the brain storms, the "flashes of genius" of the independents. But even more important is the fact that if the lone inventor is allowed to atrophy, the patent system, the institution that makes industrial research economically feasible, may become defenseless against political attack.

In this regard the patent system is already vulnerable. Not since the 1920's, when the patent pool assembled by R.C.A. brought *radio* out of its infancy, has the protection provided by patents created a totally new industry in the manner exemplified by Bell's patent on the telephone, Goodyear's patent on the vulcanization of rubber, or Hall's patent on the reduction of aluminum. The important developments of the past quarter-century (e.g., television, jet engines, synthetic fibers, detergents, rubber) have served mainly to *entrench the* established companies with large research staffs. A large portion of the U.S. population is persuaded that any monopoly, even the temporary one created by the patent grant, is fundamentally evil; and the argument that patents help "the big get bigger" has a particularly convincing ring when it coincides with evidence that the lone inventor is being denied a fair opportunity. If for no other reason than to counteract this charge, industry should give the independent inventor a helping hand.

Unplugged channels

There are many ways in which industry could help. It could, for example, give financial and moral support to the groups that are trying to strengthen the faltering patent system. More specifically, industry could subsidize adult classes at colleges and universities where independent inventors could take cram courses in the new technologies [illegible]e most effec[illegible]single act industry could perform would [illegible] its own [illegible]es toward the lone inventor.

For example, the channels of communication between corporation and inventor need to be cleared; the deluge of ideas that flood organizations like Product Development Corp. and the Institute of Inventive Research proves that these channels have been, in the past, badly plugged. Improvement, however, will not come easily. The mutual suspicion between inventor and corporation is born of bitter experiences by both. To defend themselves from crackpots looking for a lawsuit, most companies insist that the inventor at least have a patent application in process before they will listen to him. Even the Sinclair Plan included this provision, which alone may have doomed the program from the start. If the independent inventor is ever to enter the higher reaches of technology, where extensive laboratory tests are required to establish even the patentability of a nebulous idea, industry will have to find a simpler method of defending its own and the inventor's rights.

Corporations could also re-examine their policies toward their own researchers. In their employer's field of interest, these inventors are hired inventors, but in all other areas of technology, the same men are potential independent inventors. Too many companies hamper initiative by not establishing a consistent policy on the question of the "unwanted invention." Researchers should be able to exploit freely any ideas or inventions they develop that are not of interest to the company that is paying their salary.

Finally, the corporations, either directly or through industry associations, could inform the independent inventors of the types of problems that are currently proving troublesome. The National Inventors Council has used this scheme successfully for several years by periodically listing the current "needs" of the armed forces (e.g., a low-temperature storage battery for Arctic troops). The toughest part of this job for industry would be to admit occasionally that its own research programs are not infallible.

A case in point is the electronic industry's effort to find a way to make a cheap and reliable cathode-ray tube for receiving color tele-

vision. Present tubes cost ten times their black-and-white equivalents, and it will probably require an entirely new concept of design or manufacture to reduce this ratio substantially. Chances are that when the way is found, it will be so simple and obvious that every television repairman in the country will wonder why he didn't think of it first. But then, that has been true of every great invention—after it has been invented. And that is why, given a chance and enough incentive, independent inventors will always be able to pay their own way.

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New Light on the Brain

BY FRANCIS BELLO

The first five chapters of this book looked at the Who, How, and Why of research. The remaining ten are devoted to its Object. Appropriately we start with Man himself, for long before Aristotle set the locus of the human intelligence in the heart, man was longing for insight into his own thoughts.

MAN'S BRAIN, which has transformed the world and has discovered the power to destroy it, is the greatest enigma in modern science. It took at least a billion years of evolution to create the 3-pound mass, which only in the last century and a half has begun to acquire a dim understanding of its own nature. The last twenty-five years have been especially fruitful of new knowledge of the brain, thanks to a sharp rise in the volume of neurophysiological research, accompanied by swift advances in electronics and instrumentation, including the use of radioactive tracers. Indeed, there is a tantalizing prospect that research now under way will finally shed light on the elusive connecting links between man's brain and the thoughts in his head.

Since 1930 brain investigators have acquired a significant new tool in the electroencephalograph, which records electrical activity at the surface of the skull (or beneath, if the skull is open); from neurosurgeons they have learned, in great detail, how the living human brain reacts when stimulated with weak electric currents; they have had an opportunity to study the behavior of large numbers of people in whom the brain's frontal lobes have been surgically inactivated; and they have performed countless animal experiments seeking detailed answers to the central question: how does the nervous system operate?

Out of this enormous effort have come at least three major developments:

- A far-reaching new theory of consciousness.
- The recognition, tied in with the foregoing, that the cerebral cortex, or outer covering of the brain, except in a few critical regions, is surprisingly expendable.
- An attempt to formulate mathematical "models" that can explain, in some sense, how the brain works. Certain of these results convince mathematicians, at least, that every imaginable aspect of the brain's activity might conceivably be duplicated by some man-made mechanism.

Of the three developments, the new theory of consciousness is not only the most recent, having emerged in just the last few years, but the one that pulls together the greatest number of experimental observations. While many neurophysiologists had a hand in shaping the new concept, the leading contributors include Horace W. Magoun and his associates at the University of California at Los Angeles; Herbert H. Jasper and his associates at McGill University and at Montreal Neurological Institute; and Giuseppe Moruzzi of the university at Pisa, Italy. The recent findings of these and other workers seem to support a view of brain function that the brilliant neurosurgeon Wilder Penfield was perhaps the first to propose, in the late Thirties. Many details of the concept are still obscure, but

if the new view should prove substantially valid, it will mark a major turning point in man's effort to understand the brain.

Briefly, the new hypothesis argues that the neurons (nerve cells) that perform the "highest level of integration" are to be found not in the brain's outer bark, or cortex—as long supposed—but deep within the brain, in the so-called reticular system, a region that is among the most ancient from the standpoint of evolution. The term *integration* means in neurophysiology, as elsewhere, a summing up, the achieving of unity out of diversity. Recognizing that the brain is being continuously bombarded, even during sleep, with sensory impulses, the problem for science has been to discover how the brain filters out meaningful stimuli, assigns priorities among stimuli competing for attention (ignoring them all, if sufficiently fatigued), and finally makes a decision to do just one thing at a time, out of the near-infinity of possible things to do.

According to the new hypothesis, all this is achieved by a complex interchange of nerve impulses—bearing coded information—between cortical neurons and neurons in the reticular system. One task of the cortex, evidently, is to assign meanings to incoming stimuli—notably to things seen, heard, smelled, tasted, and touched—and to store these meanings, in some fashion, for future reference. The cortex also, it seems, transmits some sort of condensed, edited, and annotated version of the flood of incoming sense stimuli into the reticular system, where the final integrating process takes place. This final integration may lead the reticular system to issue impulses that will "arouse" appropriate sensorimotor regions of the cortex to initiate a type of muscular response that has proved successful under similar circumstances in the past—or, alternately, it may call upon some part of the brain (probably the cortex) to give more "thought" to the problem.

It is perhaps too early to say whether this sketchy account of the role of the reticular system is roughly correct or quite mistaken. If essentially correct, it goes far toward explaining the unity of the

central stream of consciousness. Even so, however, it sheds virtually no light on such equally basic problems as: what, really, takes place in learning; what is memory; what is intelligence; and how does the brain recognize "universals"? The last problem, a favorite of Gestalt psychologists, asks how it is that we learn to recognize a circle, a triangle, or a spoken word, even though each may be highly distorted. One important fact demonstrated in the last twenty-five years is that visual recognition of universals involves a very complex learning process. People who have been blind from birth and have later gained their sight require weeks to recognize even simple objects; and once they have learned to distinguish, say, an egg from a cube of sugar, they become utterly confused again if the two are presented under a colored light.

Thus, despite the new hypothetical role of the reticular system, anatomical and physiological studies of the brain have been of little help to the understanding of thought processes. In recognition of the difficulty of erecting any sort of bridge between physiology and psychology, the prevailing tendency among psychologists has been to regard man as an "empty organism" and his brain as a "black box."

In this view, about all man can aspire to do is to investigate the brain's input and output and, with luck, discover meaningful correlations. On the other hand, neurophysiologists (and a few psychologists and psychiatrists) hope that a knowledge of the brain's inner workings will shed useful light on the nature of intelligence, on how the brain learns, finds its satisfactions, and why, on occasion, it breaks down. They would like to know, for example, whether schizophrenia is primarily physiological in origin—traceable, perhaps, to a disturbance in an enzyme system or to a harmful chemical compound substituting for a needed one—or whether the ailment is basically a malfunction in information processing, peculiar to computers containing billions of switching components.

There is, of course, no certainty that the brain is clever enough to understand itself, for most of the hard problems in science reach a climax in the nervous system. To understand the brain it will first be necessary to know much more than is now known about protein chemistry, biochemistry (including the role of hormones), biophysics, the advanced mathematics of large random ensembles, and the new semiscience of information theory. Moreover, in the brain science comes face to face with indeterminacy—or uncertainty—on a macro scale. It will never be possible to know precisely what 10 billion nerve cells are doing at any one instant. If researchers try to stick too many recording elements into the nervous system, it will simply stop working.

The mind misplaced

From today's perspective it is difficult to appreciate how long man remained ignorant of the function of the brain and nervous system. Although Hippocrates knew better, Aristotle, with his great influence, convinced a great many people that the heart was the organ of thought. In the second century A.D., Galen, illustrious Greek physician and anatomist, corrected this mistake of 18 inches in localization, and went on to declare that he had found the reservoir of the soul's "animal spirits" in the brain's hollow chambers, the ventricles, which are filled with a clear watery fluid. Galen proposed that these spirits, presumably in solution, were pumped by the brain to all parts of the body.

Galen's views went virtually unchallenged until the late seventeenth century, when Thomas Willis, an anatomist and physician, asserted that the brain's gray matter was the true lodging of the "animal spirits," and that they traveled about the body through a network of nerve fibers.

The soul's "animal spirits" were at first incorporeal and mysterious. The early microscopists, however, declared them to be a

subtle juice, which they could see oozing from severed nerve fibers. It was this juice, said one (Malpighi), that transmitted sensations hydraulically from sense organs to the brain.

The first hint of the electrical nature of nerve sensation was not discovered until 1791, when Luigi Galvani, of Bologna, reported that the leg of a dead frog twitched under an electric current. The belief in soul-like "animal spirits" faded soon after 1800, and the stage was set for the modern study of the nervous system.

The great unknowns

The great physiologists who advanced understanding of the brain and nervous system in the busy and wonderful nineteenth century never achieved the popular fame of their scientific colleagues in chemistry and physics.

One scientist of tremendous insight was Hughlings Jackson, an English neurologist who lived between 1834 and 1911. Modern students are still amazed by the many shrewd inferences about brain function that Jackson drew from close study of epilepsy and other disorders of the nervous system, but his methods were soon overshadowed by more fruitful techniques of direct experimentation.

In the 1870's Sir David Ferrier, a Scottish physiologist, was among the first to apply electric currents directly to the exposed cortex of monkeys. He found that electrostimulation of specific regions of the so-called "motor" cortex would cause a monkey to move muscles in various parts of its body. About the same time Friedrich Goltz of Strasbourg made the then surprising discovery that a dog could lead a fairly active life even with its entire cerebrum removed. It was a "mindless" robot that could display no memory, no emotion (except a crude type of anger), and none of a dog's familiar "purposeful" behavior.

Still another line of investigation was devised by anatomists who had long been balked in their efforts to trace individual nerve fibers through the spinal column and into the brain. They found that if

a nerve fiber were cut, it would quickly degenerate along its entire length, leaving a distinctive trail that could, after staining, be traced under a microscope.

Not all the great brain investigators of the late nineteenth and early twentieth centuries worked in obscurity. Ivan Pavlov earned a Nobel prize in 1904 (for his research on digestion), and later influenced a generation of educators—and the whole Soviet system—with his remarkable demonstrations of the conditioned reflex in dogs. But although he was a physiologist, Pavlov might just as well have been conditioning "black boxes" as living animals, for he had no more success than anyone else in finding the mechanism by which the brain makes the associations so fundamental to learning.

The great integrator

The man who is credited above all others with crystallizing the modern science of neurophysiology is Sir Charles Scott Sherrington. His first publications appeared in 1884, his last sixty-seven years later, in 1951, when he was over ninety.

Sherrington's major work was in elucidating what he liked to call "the integrative action of the nervous system," the process whereby the nervous system handles a multitude of incoming stimuli and "unifies from separate organs an animal possessing solidarity, an individual . . ." This is the same problem, of course, that the new reticular theory of consciousness seeks to answer at the "highest level." Sherrington's primary concern was with the neuronal integrations that take place in the spinal cord.

One of the great landmarks in neurophysiology was the series of lectures given by Sherrington at Yale in 1904. "The results before you," he said toward the close of his lectures, "must appear a meager contribution toward the greater problems of the working of the brain; their very poverty may help to emphasize the necessity for resorting to new methods of experimental inquiry in order to advance in this field."

Sherrington said he saw particular promise in psychological experiments, then just beginning, in which sections of brain were systematically removed from animals previously taught to perform acts of reasonable complexity, such as running a maze.

The brain's resourcefulness

By 1929 one group of these experiments, conducted by Harvard psychologist Karl S. Lashley, was essentially complete. Lashley's findings were as astonishing as anything reported in science in this century.

Lashley found that in rats which had been trained to thread a maze he could slice away virtually any part of the cortex (so long as he did not destroy vision or paralyze the animal) without destroying the learned behavior.

These findings led to two general hypotheses: (1) that memory "traces" were scattered so diffusely throughout the cortex that so long as a fair amount of it remained intact, memory and learning were not destroyed; or (2) that memories were stored deep in the mid- or "primitive" brain, into which Lashley could not cut without destroying consciousness itself.

Prior to Lashley's work, neurophysiologists (including Sherrington) believed that the billions of neurons in the central nervous system were inactive most of the time; that limited neuronal networks were activated in response to particular stimuli; and that somehow, simple conditioned-reflex arcs, of the Pavlovian type, were created through the process of learning.

Today, in the light of the new theory of consciousness, it appears that both cortex and midbrain participate in storing patterns of learned behavior. One suggestion is that the cortex may play a vital role in the learning of skilled motor acts, such as playing a piano, but that in time some of the cortical pathways may be "short-circuited," permitting the reticular system and subcortical motor centers to take over more of the job.

What price cortex?

Not long after Lashley reported retention of learning in partially brainless rats, neurosurgeons and psychiatrists confirmed his results, at least in part, in humans. The main confirmation came with the adoption of prefrontal lobotomy (or leucotomy) as a treatment for mental illness, chiefly schizophrenia, and for relieving intractable pain, especially that induced by cancer. The Portuguese neurologist, Egas Moniz, sponsored the first human lobotomy in 1935 after hearing that the operation had relieved a highly trained chimpanzee, Becky, of temper tantrums brought on by frustration.*

After 15,000 to 20,000 lobotomies, psychiatrists and psychologists are still not sure what effect slicing away a billion or so brain cells has on behavior. A number of patients have actually scored higher on intelligence tests after lobotomy than before, and there is one case on record of an I.Q. of 160 after the removal of one lobe. This, of course, is not so inexplicable as it may appear, for few patients are ideal test subjects before surgery. The most noticeable effect of lobotomy is on personality, and the direction of change is almost always in the direction of reducing inhibitions and sense of responsibility.

Electromapping the brain

About the time that Lashley was reporting his findings, Hans Berger, a German psychiatrist, announced that he could detect faint "brain waves" with electrodes placed on the scalp. The electroen-

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cephalograph, or EEG, is now used routinely to classify types of epilepsy and as an aid in establishing the location of brain tumors. The EEG has also contributed greatly to the new reticular theory of consciousness. So far, however, there is no reliable evidence that brain waves provide any clue to intelligence or personality traits, or that they undergo changes as a result of mental illness.

In the early Thirties, Walter R. Hess, a Swiss neurophysiologist, devised methods for mapping, by electrostimulation, regions deep within the brains of unanesthetized, freely moving cats. Through a small hole in the cat's skull he was able to insert electrodes reaching to the diencephalon, or "in-between-brain," a region containing part of the reticular system. Hess's findings, which brought him a Nobel award in 1949, established that the diencephalon is the control center for the autonomic nervous system—the system that regulates the workings of the heart and other internal organs.

Whereas electrostimulation of the brain's surface evokes only isolated muscular movements or twitches, Hess found that by stimulating the diencephalon he could actually make his cats "do something." They could be made to show abnormal hunger or thirst, act as if they had seen an unfriendly dog, pant, sneeze, urinate, or even curl up and go to sleep.

Recently James Olds of McGill University has discovered that stimulation of a certain region in the brain of a rat seems to evoke an exceedingly pleasurable sensation. If the rat is allowed to switch on the current by stepping on a pedal, he quickly learns what to do and spends many happy hours stimulating himself, even in preference to eating.

Nerve and its signals

Many of the eminent neurophysiologists of this century have felt that the proper place to begin the study of the nervous system is with the nerves themselves.

In 1912 E. D. Adrian, the English Nobel laureate, and Keith

Lucas discovered that nerves always fire on an "all-or-nothing" basis, which means that if a nerve is in condition to fire, and is adequately stimulated, the size of response and the speed of conduction are independent of the size of the stimulus. Later Adrian showed that the nerve's method of signaling that a stimulus has increased in size is to fire more rapidly.

A central problem, still much a mystery, is how the nerve impulse is transmitted across a "synapse," the junction, or small gap, where the end fibers of one nerve cell meet another cell (or neuron). It can be shown that the arrival of certain impulses may inhibit a cell from firing, even though the cell may otherwise appear to be adequately stimulated. The nature of inhibition, so crucial to the operation of the whole nervous system, is now a center of controversy.

One school holds that inhibition may be explained by release of chemicals that "paralyze" the neuron and keep it from firing. An unidentified chemical with the necessary properties has recently been discovered. Another school holds that inhibition may be explained, in large part, by electrical "roadblocks" that are set up when an impulse tries to squeeze through the ultrafine branches at the end of some nerve fibers. As an impulse "dies out" in these branches, it seems able to generate a field effect that can "block" impulses passing through nearby fibers. When this happens, a neuron that might otherwise receive enough impulses to fire, fails to fire—i.e., is "inhibited"—because the impulses never arrive in suitable strength.

The "model builders"

Among brain investigators, two of the scientists associated with the new inhibition theory, Warren McCulloch and Walter Pitts, both M.I.T. researchers, have had a great influence on modern efforts to guess how the brain operates. This guessing, or model-making, is an inseparable part of all research. In discussing this matter with other brain investigators, Pitts said: "I think model-makers in general really have two functions. First they want to

demonstrate that thus and such a function, which various people suppose can be done only by the mind's substance or some other nonphysiological entity, not by any mechanism, can in fact be done by some mechanism. . . . As soon as the psychologists know exactly what it is they want the brain or the mind in question to do, we can certainly build a machine to do it. . . . For us it is superfluous, but it *is extremely important with respect to the rest of the world.* The second function of modelmakers is to find models that throw a light [on] how in fact the brain does something."

In 1943 McCulloch and Pitts demonstrated mathematically how networks of "neurons" having certain formal properties could produce any desired output (i.e., "behavior"), provided the output could be described "completely and unambiguously" in words. A few years ago John von Neumann, the eminent mathematician, since named to the AEC, paid his respects to the McCulloch-Pitts theory. "It has often been claimed," said von Neumann, "that the activities and functions of the human nervous system are so complicated that no ordinary mechanism could possibly perform them . . . that such functions . . . are per se unable of mechanical, neural realization. The McCulloch-Pitts result puts an end to this."

Von Neumann then went on to discuss the implications and limitations of the theory. He began by asserting that "there is no doubt that any special phase of any conceivable form of behavior can be described 'completely and unambiguously' in words." He hastened to add, however, that to describe and define how human vision works might "require thousands or millions or altogether impractical numbers of volumes."

From this von Neumann concludes that "it is perfectly possible that the simplest and only practical way actually to say what constitutes visual analogy [i.e., human vision] consists in giving a description of the connections of the visual brain . . . Obviously, there is on this level no more profit in the McCulloch-Pitts result."

No one should be disappointed to learn that the McCulloch-

Pitts theory would not lead to a mechanical brain. It was never meant to. Its basic purpose was to show mathematically that the living brain could work on purely physical principles, and this, evidently, it accomplished.

The need for randomness

But this is to consider only the first of the two functions of modelmaking. The second function, that of throwing light on brain mechanism in advance of discovering the brain's wiring diagram, is really the important one. The British mathematician Donald M. MacKay writes: "The considerable effort going into this theoretical modelmaking is justified chiefly by the hope that out of it may come a way of describing the thinking process, sufficiently close to psychiatric realities to be useful in diagnosis, yet sufficiently operational and objective to allow the physiologist to make his maximum contribution to the study and treatment of mental illness."

What one can read here between the lines is an obvious dissatisfaction with the present terminology of psychiatry. Sherrington thought it amusing that Freud's study of the mind was as completely divorced from the anatomy and physiology of the brain as was Aristotle's over two thousand years earlier.

An outstanding feature of the models now visualized by both Pitts and MacKay is that the fine details of their neuronal connections shall be essentially random, for it is difficult to imagine that the nervous system could be put together in any other way. Recent estimates make it extremely unlikely that enough "information" could be coded into the genes to specify all the connections of the nervous system's 10 billion neurons. Presumably randomness reaches a climax in the cerebral cortex, where neurons are packed some 15,000 to the cubic millimeter.

To construct a useful mathematical model of a random nerve network has turned out to be enormously difficult. At the same

time, such a model promises to circumvent the impasse that arises when, as von Neumann indicated, one tries to build a model based on the early McCulloch-Pitts concept. Since the random model would involve no more information than the genes can carry (probably under 10 billion "bits") it would no longer be necessary to describe mental behavior "completely and unambiguously" in thousands or millions of volumes of words.

Memory's secret trace

"Unfortunately," Professor Adrian told the British Association for the Advancement of Science, in his presidential address, "it is a great deal easier to study the immediate reactions of the nervous system than the more persistent changes which alter its habits and give us our memories. We know next to nothing about the plasticity which is the most important feature of the brain . . ."

One hypothesis popular about ten years ago suggested that a memory trace might take the form of nerve impulses "reverberating" or chasing themselves around closed loops of neurons. This notion has since been tested by teaching rats to thread a maze, then subjecting their brains to violent electroshocks, presumably adequate to destroy any reverberations that might exist. When the shocks were given within half an hour after learning, all memory of the learned task seemed to be erased. Beyond a half hour, however, shocks had no effect. This suggests that reverberating impulses may, in some fashion, "imprint" lasting changes in the brain cells—conceivably by modifying slightly the actual protein molecules of which the cells are composed.

Other hypotheses attribute the brain's plasticity to swelling of nerve fibers through repeated stimulation, or to similar growth (or strengthening) of the synaptic "knobs" (the relay points where nerve impulses presumably pass from one nerve to another), or to the actual growth of new "knobs" where none existed before.

What is intelligence?

And what of "intelligence" itself? On this aspect of the brain's plasticity, modern neurophysiology is silent. It seems probable, however, that this century's preoccupation with intelligence testing has done inestimable mischief. The brain is far too subtle a mechanism to be stamped "I.Q.: such-and-such."

Nevertheless, it is true that I.Q. tests must measure something. Although E. L. Thorndike once proposed that intelligence be conceived of as "that which intelligence tests measure," most psychologists would prefer to have tests that, in some sense, measure so-called "biological" or "native" intelligence. It is doubtful if these have yet been devised. In any event, existing tests have shown that identical twins—even those reared apart—consistently achieve much closer scores than fraternal twins. There is thus no longer much doubt that intelligence, whatever it may be, contains a strong genetic component.*

What may be the first correlation between "intelligence" and some physiological (and perhaps genetically determined) aspect of the brain was reported in December, 1954, by University of California psychologists under David Krech. They find that "maze-bright" strains of rats have more cholinesterase (an enzyme) in certain areas of the cortex than do "maze-dull" animals. If this finding can be confirmed, it opens up a momentous new line of research.

Free will and all that

Modern science spends little time arguing the "mind-body" problem, dear to generations of philosophers, or debating "free will."

* *A strong genetic factor in mental illness has also been shown by Franz J. Kallmann of the New York State Psychiatric Institute. From studying thousands of twins, he notes that whenever he discovers an identical twin with schizophrenia, the chances are about 85 out of 100 that he will find the other twin similarly affected. (Identical twins, of course, are no more susceptible to mental illness than is the general population.)*

The tendency is to regard these two issues as semantic rather than real.

The distinguished American neurosurgeon and long-time student of the brain, Percival Bailey, expresses a widely held point of view when he says: "To my way of thinking, mind is simply a name which we give to the functioning of the cerebral cortex."

Some people have thought the "free-will" debate settled in favor of "freedom" by the theories of modern atomic physics, which argue the fruitlessness of trying to establish ultimate causal relationships between events. However, it would seem that this argument would lead not to "freedom of choice" but to chaos. It seems inconceivable that each of our countless daily decisions is settled by the unpredictable flopping of an electron somewhere in the nervous system.

The view of the scientist (at least) is clearly summed up by Sherrington: "If 'free will' means a series of events in which at some point the succeeding is not conditioned by reaction with the preceding, such an anomaly in the brain's series of events is scientifically unthinkable."

Presumably not everyone will be pleased to regard his brain as a glorified computer. Sherrington puts it more gracefully. In the preface to the second edition of *Man on his Nature*, published when he was ninety-three, he wrote: "The book stresses the view that man is a product, like so much else, of the play of natural forces acting on the material and under the conditions past and present obtaining on the surface of our planet."

One thing is certain: if the Great Automaton is ever built—complete with 10 billion neuron-like switching units—it will still lack something. Some will call it a soul. Others will be satisfied if it is simply called compassion. They will suggest that man owes whatever compassion he may possess, not to his soul or his stars, but to a million human experiences of every quality and description. From such experiences, automata are forever excluded.

January, 1955

7

How Are We Fixed for Water?

BY FRANCIS BELLO

Of all the necessities of man, water is the most vital and, therefore, a fitting subject for major research. Here are the facts of a situation often clouded with fiction. The U.S. water supply—so essential to its technology—makes a checkered but encouraging pattern.

TO MAINTAIN A U.S. CITIZEN in the manner to which he is accustomed requires the deliberate use of about 1,500 tons of fresh water a year. (Of all other materials—food, fuel, metals, plastics, lumber, sand, and gravel—he requires only about 18 tons.) Of the 1,500 tons of water, about 500 vanishes into thin air supporting the growth of food and fibers (chiefly cotton). Most of the remaining 1,000 tons does not evaporate, but returns, more or less polluted, to streams and rivers, after having passed through the nation's

homes, and through more than a quarter of a million mines, factories, and steam-power plants.

This 1,500 tons of water per person per year includes only water drawn from rivers and lakes or pumped from the ground. It does not include 10,000 tons (per capita) channeled through hydropower plants, or the thousands of tons that fall freely on forests and farms, and, running to the sea, make possible river navigation, pleasure boating, fishing, and swimming.

While water thus outweighs everything else consumed in the nation, it is scarcely discernible in the family budget. Very little of it costs as much as a nickel a ton. In a $365-billion economy the total U.S. water bill probably does not exceed $3 billion a year. (The city dwellers' share is about $500 million, agriculture's perhaps $200 million; the rest of the cost is borne by industry.)

Nevertheless, the total U.S. investment in all types of water-management facilities is considerable—roughly $40 billion. About one-fourth of this sum has been contributed by the federal government, which, prior to the Eisenhower Administration, had fairly firm plans for spending $25 billion more, and less definite plans for another $25 billion.

All water experts agree that the U.S. does not know all it should about its water supply or its water requirements. Many experts believe that, for lack of this information, a substantial amount of money has already been misspent and much more may be misspent in the years ahead.

Is the U.S. running out of water? Not at all. But the seventeen western states are using about 70 per cent of all the water they may expect to develop at reasonable cost. Arizona's water use already exceeds the dependable supply available within its borders, and Texans are talking of importing water from the Mississippi.

The conclusion of President Truman's Materials Policy Commission was that by 1975 the U.S. may be using as much as 90 per cent of all the water that can be developed at reasonable cost. But

it is important to recognize that "use" may be "consumptive" or "nonconsumptive." Industrial and municipal use is essentially "nonconsumptive" in that over 90 per cent of the water "used" in cities and in most plants is ultimately discharged and can be used again. Irrigation, however, is a big consumptive user, for about two-thirds of the water used on crops either evaporates directly or transpires through the leaves of plants and is lost.

The commission estimates that between 1950 and 1975 the total U.S. demand for water may nearly double. Irrigation, however, is expected to increase relatively little. Over 85 per cent of the new demand presumably will come from industry and from cities—users that return most of the water they require. Over 50 per cent of the total new demand foreseen by the commission may represent the cooling-water requirements of just one type of plant, electric-generating stations, which are expected to quadruple in capacity. Fortunately, power plants can get along on brackish water, or if necessary on badly polluted water.

Over the next twenty-five years industry will not find all the water it wants everywhere in the U.S., but it should still find good supplies in most areas where expansion is logical. (Texas and Southern California may be important exceptions.) A survey by *Fortune* shows that among twenty-five big utilities, steel, pulp and paper, oil, and chemical companies (which appear to use about *one-third* of all the water required by industry) only one firm believed it would have trouble finding the water needed for a 50 per cent expansion.

The following two pages contrast the ways in which the eastern and western U.S. use water, and underscore the great inequality in natural supply between the two regions.

In the West, the Limits

The nation's heaviest single withdrawer and *consumer* of water is irrigation. The figures (based on a 1950 study by the U.S. Geological Survey) show that the seventeen western states use about 77 bgd (billion gallons per day) for irrigation, or nearly as much as the thirty-one eastern states use for *all* purposes. The West gets almost one-fourth of its water from wells, the East only an eighth. (State water-use figures exclude brackish water.)

The chart shows how the nation's average runoff of 1,250 bgd is divided between West and East. (The runoff is the water left after evaporation and plant transpiration have taken their toll of the average precipitation of 4,300 bgd.) Owing to irrigation

The U. S. demand for water

	West	East
Municipal and rural	5	12
Industrial	3	65*
Irrigation	77	3
Total	85	80

*plus about 15 bgd of brackish water

(in billion gallons per day)

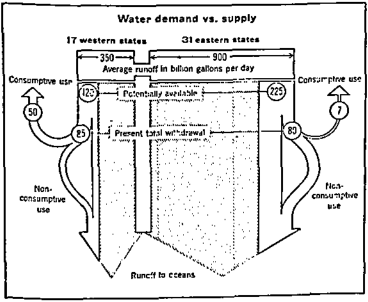
The biggest water users: (billions of gallons per day)

West		East	
California	22.3	Illinois	9.9
Idaho	13.8	New York	9.6
Colorado	8.9	Ohio	9.3
Arizona	6.0	Pennsylvania	7.0
Texas	5.9	Michigan	5.9
Montana	5.1	West Virginia	3.5
Washington	4.4	Virginia	3.2
New Mexico	3.4	Louisiana	3.1
Utah	3.3	Tennessee	2.6
Wyoming	2.9	Indiana	2.4

of the Supply Are in Sight

losses, which probably cannot be reduced significantly, the West's consumptive use of 50 bgd is over seven times that of the East. In fact, but for irrigation the nation's consumptive use of water would be insignificant. The chart also shows that the West is much closer to the estimated limits of its potentially (i.e., economically) available supply than is the East.

About two-thirds of the West's average runoff of 350 bgd is concentrated in the Pacific Northwest, hence is of no value to the arid Southwest. If the Southwest is to grow industrially, it will need plants that use water with great frugality.



Surface water: in search of a policy

No other aspect of water conservation raises so many controversial issues as river-basin development. In his January, 1954, State of the Union message, President Eisenhower announced that he would submit to Congress a "uniform and consistent water-resources policy." He promised, meanwhile, that the federal government would continue to support construction of irrigation, flood-control, and power projects "wherever [they] are beyond the capacity of local initiative, public or private." He also said that he favors more attention to upstream flood-control measures.

Most of the great western dams built in this century have had two primary purposes: to provide water for irrigation and (since 1936) to control floods. Power, of course, was often a welcome bonus. It is now becoming apparent that industry's need for water should be given as high a priority as agriculture's, particularly since irrigation represents a relatively uneconomic use of water. (Manufacturing produces about fifty times as much product value per gallon of water withdrawn as does irrigation.) It is also becoming

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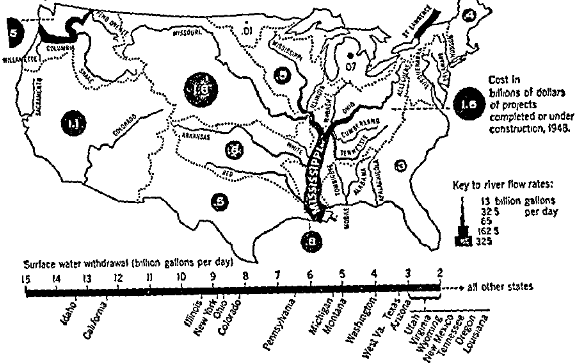
Billions for River Basins

[below and at right]

Two-thirds of the water that runs off to the sea is carried in twenty-five large rivers. In the basins of the largest of these rivers the U.S. Government has invested about $10 billion in irrigation, flood-control, navigation, and power projects. Of the total, the Corps of Engineers has spent about $5.7 billion, Bureau of Reclamation about $2.5 billion, and TVA about $1.6 billion. The investment breakdown by basins, as shown on the map, is for 1948. (Later figures are not available.) The Corps, Reclamation, and the Department of Agriculture, a newcomer to river management, have plans for spending billions more developing the nation's water resources. These plans, however, may have to be revised substantially in the light of future government policy.

• River basins • The Federal Investment • Use by states



evident that there is no practical way to control extraordinary floods, e.g., the great Kansas City flood of 1951. The unpleasant fact is that if people insist on occupying a major flood plain they must expect to be inundated periodically.

Ground water: too little information

In the last twenty years the nation has increased its withdrawals of ground water over 300 per cent until it now obtains about 30 bgd, or nearly a fifth of its total supply, from wells. Over 55 per cent of the 30 bgd is pumped by five states, California, Arizona, Texas, Louisiana, and Arkansas.

That water tables are declining in many parts of the country is not necessarily alarming. Decline accompanies any continued withdrawal and will level off, *provided* average draft does not exceed average long-term inflow. In some cases pumping actually increases the inflow to a ground-water reservoir.

Even the mining of ground water is not, in itself, sinful, or even

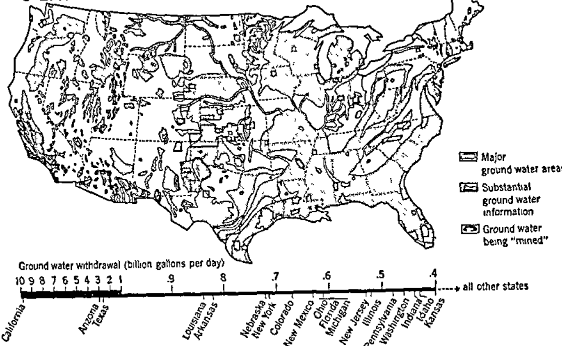
Pumps Running Overtime

[below and at right]

Ground water is, as a rule, water in transit. Nearly half of all water reaching the ocean makes part of its trip underground. The shaded areas in the map roughly indicate underground reservoirs, which, filled over the centuries, now hold more water than the Great Lakes, and perhaps as much water as thirty-five years of average runoff. Only in the areas outlined in black, however, have geologists made a thorough study of ground-water resources.

The heavily shaded areas denote the danger spots, where water is being "mined" or withdrawn faster than it is being replenished. In the San Joaquin Valley, California, some 40,000 wells produce over 6 bgd of water (about one-fifth of all the ground water used in the nation) and the overdraft probably exceeds 1 bgd.

• Ground water • Where it is being "mined" • Use by states

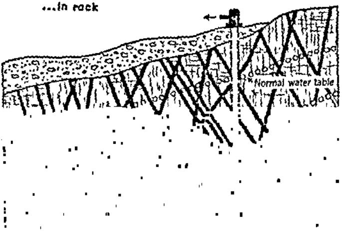


Underground Reservoirs

Some of the finest ground-water reservoirs are found adjacent to rivers that lie in a broad alluvial bed of sand and gravel. The diagram shows how pumping may draw water from the stream into the reservoir, purifying it on the way.

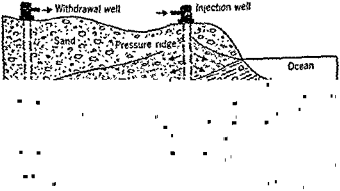
Ground water: in river valleys

...in rock



Ground water may even be found in rock, provided the rock contains pores or channels through which water may seep freely. Rocks containing channels provide the water supply for suburban Atlanta and other Piedmont communities.

...along the sea coast



In many coastal regions overpumping of ground water has permitted salt water to move inland. California has considered checking the intrusion by erecting a barrier of fresh water (possibly treated sewage).

imprudent—provided the situation is recognized. There is no reason why a rich pocket of water, any more than a rich vein of ore, should be conserved for posterity. Some water-mining areas have plans for recharging ground-water reservoirs (e.g., by diverting excess surface water underground) or for turning to alternative sources of supply. Other areas, notably the High Plains of Texas, where withdrawal is perhaps thirty times inflow, have not yet figured out what to do when the wells run dry. In some cases there may be no choice but to let the local economies decline. The present total overdraft in the Southwest is perhaps in the neighborhood of 5 billion gallons *per day.*

Pollution: a big cleanup needed

From one point of view, and a highly vocal one, the pollution of U.S. rivers has become a national shame. From another, less vocal, their pollution has been an unattractive but probably unavoidable concomitant of industrial growth. The fact is, the transportation and dilution of wastes is one of the major functions of water. Most public-health experts agree that it is uneconomic (and unreasonable) to ask for waste treatment for treatment's sake. The purpose of treatment, they conclude, is to meet the broad public interest, that is, to keep a river from growing so polluted that it can no longer serve the legitimate needs of neighboring towns and industries.

In the last half-dozen years there has been a notable upsurge in public awareness of pollution. Over two-thirds of the states have enacted reasonably uniform legislation on the matter, and many towns and industries have been ordered to clean up. But in many important river basins a real start has yet to be made. A few states (mostly southern) even capitalize on their laxity toward pollution to attract new industry.

The present municipal investment in sewage-treatment works is over $5 billion. The U.S. Public Health Service estimates that cities and industries would have to spend about $10 billion over the next

ten years to make the nation's rivers clean enough to meet the "broad public interest."

Water for drinking: too little and too late

In 1953 about 1,000 U.S. towns and cities had trouble supplying consumers with enough water. The problem in a great many cases was not a lack of water but a lack of treating or distributing facilities. About 70 per cent of the nation's 16,750 waterworks—supplying 100 million people—are publicly owned, hence are enlarged usually only after much public (and political) travail. For the last hundred years private utilities have been getting out of the water business, primarily, it appears, because the ratio of capital investment to sales is unattractive. Most waterworks now operating cost about $75 per person served; yet the average annual water bill is only $8 per person, including water for commercial use.

Some scientists familiar with the progress being made in purifying sea water by use of ion-exchange membranes (thin plastic sheets which use electric power to concentrate saline water on one side, desalted water on the other) are inclined to doubt that any coastal city will ever again build aqueducts as long as those supplying Los Angeles. They estimate that the membrane method, still in early development, may produce fresh water from the sea for less than 30 cents per 1,000 gallons, or very little more than domestic consumers now pay. Distillation is more expensive.

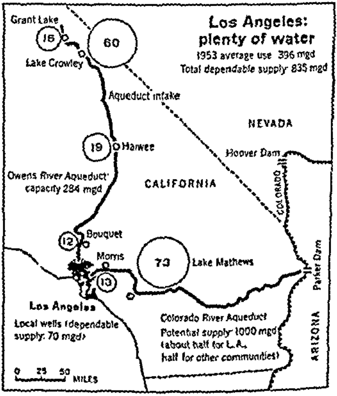
How is industry fixed for water?

Five industries—steam-electric power, steel, chemicals, petroleum refining, and pulp and paper—probably use about 80 per cent of industry's total estimated water demand. To discover how much difficulty these five groups had in finding water for their 1946–53 expansion programs, and how much difficulty they anticipate, *Fortune* directed a questionnaire to several of the biggest companies in each of the five groups.

The overwhelming impression created by the replies is that, with

Water for Drinking:

No other city in the U.S. has built up so great a surplus water supply as has Los Angeles. Part of the supply, however, is in doubt. The Supreme Court is now considering whether or not Southern California has first call on 1,000 mgd from the Colorado River. Even should the decision be unfavorable, however, Los Angeles expects many years to pass before Colorado, Arizona, and Nevada develop a demand for the disputed water.



a $7.5-Billion Investment

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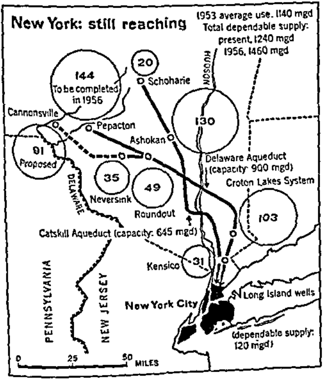
expects to reach out to the west branch of the Delaware, despite the opinion of an expert panel that Hudson water (filtered and purified) would be cheaper.



Reservoirs (capacity in billion gallons)

few exceptions, the biggest industrial water users have not encountered serious water shortages and probably will not in the foreseeable future.

The water requirements (both fresh and brackish) of these five industries in 1950 and their estimated 1975 demand are given below. (The figures for the chemical industry are a *Fortune* estimate; all others are from the report of the Materials Policy Commission.)

	Billion gallons per day 1950	1975	*Per cent increase*
Steam power	35*	125	257
Steel	13	20	54
Chemicals	10	30	200
Petroleum refining	7	15	114
Pulp and paper	4	8	100
	69	198	

* *Steam-power plants probably used most of the 15 bgd of brackish water used by industry in 1950.*

The six utilities, four steel companies, six oil companies, five chemical companies, and five paper companies that responded to the survey have a combined water requirement of 29 bgd, or roughly one-third of industry's total estimated water demand. The breakdown of the 29 bgd, the average increase in water demand over 1945, and the average increase in productive capacity over the same period follow:

	1953 demand (bgd)		*Average per cent increase over 1945*	
	Fresh	*Brackish*	*Water demand*	*Prod. capac.*
Steam power	12.7	5.3	65	90
Steel	3.7	.4	35	33
Chemicals	2.5	1.7	100	114
Petroleum	1.0	.9	20	59
Pulp and paper	.7	.05	72	107

It is evident that water demand (except in the steel industry) did not rise so rapidly as productive capacity. In response to another question, seventeen of the firms reported that plants they had built since 1945 were indeed designed to use less water than similar plants built before the war. These seventeen firms said they had reduced typical water requirements as follows: utilities, 15 to 55 per cent; refineries, 16 to 95 per cent; chemical firms, 3 to 33 per cent; pulp and paper firms, 15 to 44 per cent. Only one steel company reported any reduction (12 per cent). That the steel companies as a group increased their water demand more than plant capacity implies that newer steelmaking processes tend to require more cooling water than do older ones.

Since the Materials Policy Commission reported that plans for about 300 World War II facilities of various sorts had to be abandoned or modified for lack of water, *Fortune* asked: "During your company's expansion program, 1945–53, did you find the problem of obtaining adequate water supplies (*a*) frequently very difficult, (*b*) difficult in a few cases, (*c*) not particularly difficult?" The replies:

a. *Frequently very difficult*	*2 companies*
b. *Difficult in a few cases*	*6 companies*
c. *Not particularly difficult*	*18 companies*

The two firms checking *a* were a chemical firm and a utility. Of the firms checking *b*, half were pulp and paper firms. Two other firms in this industry checked *c*.

Two firms said that for lack of water they had to abandon plans for building in regions of first choice. One was the chemical firm that checked *a*, which reported that it could not find the water it needed around Corpus Christi, Texas; the other was a paper firm that could not find sufficient water in the Appalachian area (state not specified).

Fortune next asked: "Assuming your company should wish to

expand 50 per cent over the next ten to fifteen years, how much difficulty do you think you would have finding the water needed?" Answers:

a. *We would have no particular difficulty obtaining water—at close to present costs—just about anywhere it would be logical for us to expand.* .... 10 *companies*
b. *While water would be tight in some areas, we are confident that by designing for more efficient use of water than is economic today, we should be able to get all the water we would need at a reasonable cost.* .... 15 *companies*
c. *In some vital areas we do not know at present how we could obtain the water we would need at a price we could afford.* 1 *company*

Firms checking *a* included representatives of all industries except chemicals, which checked *b*. The one firm checking *c* was a petroleum company that described the vital areas as "Southern California (near Los Angeles) and possibly Texas (Houston Ship Channel area)."

To discover how much plant expansion might still be possible near existing *fresh-water* plant sites, *Fortune* asked: "In what per cent of your major plant locations could a 'duplicate' plant be built next door (i.e., within a few miles) with the assurance that it could obtain water in the *quantity* and of the *quality* desired?" Answers:

a. *In perhaps 85 per cent of the localities*	14 *companies*
b. *In at least half of the localities*	7 *companies*
c. *In relatively few localities*	4 *companies*

One seaboard utility did not qualify as a fresh-water user; otherwise all utilities but one checked *a*. One utility, and one petroleum, one chemical, and one paper firm checked c.

To learn how much the use of water might be cut by a rise in the cost of water, *Fortune* asked: "If water were to become, say, *twice* as expensive as it is today (at one of your newer plants), presum-

ably it would pay you to increase the amount of water re-used or recycled. Under such circumstances, by about what per cent could you reduce the plant's demand for *fresh* water?"

	Percentage	
	Minimum	*Maximum*
Steam power	0	95
Steel	0	40
Chemicals	10	60
Petroleum	0	12
Pulp and paper	0	25

The surprising 95 per cent reported by one major utility is particularly significant since it indicates that the industry using nearly three times as much water as any other could, if pressed, make a drastic reduction in its water requirements. Next *Fortune* asked: "Approximately what per cent of the *fresh* water entering your plants is discharged again and is potentially available for re-use elsewhere?"

	Percentage		
	Minimum	*Maximum*	*Weighted average*
Steam power	40	100	99.8
Steel	93	100	96
Chemicals	*none*	98	82
Petroleum	*none*	95	89
Pulp and paper	80	100	92

The low value of 40 per cent was reported by a utility in the Southwest. The answer "none" was reported by an oil company for its refineries along the east coast (which require very little fresh water) and by a chemical company for its large Texas plant. One can conclude that where fresh water is precious, it is used until it disappears through evaporation. It would thus seem that, if neces-

sary, industry could reduce its demand for fresh water well below the estimates given in the previous question.

The final question dealt with pollution: "Roughly how much has your firm spent since the end of World War II on water-pollution abatement?"

	Minimum	*Maximum*	*Average*
Steel (two replies)	$3,000,000	$50,000,000	——
Chemicals	460,000	10,000,000	$4,500,000
Petroleum	2,500,000	9,000,000	4,700,000
Pulp and paper	400,000	3,300,000	1,500,000
Steam power (not considered polluters, but one firm spent $1,500,000)			

The total for all firms covered in the survey was over $110 million. It is clear, however, that for the smallest spenders the average *yearly* expenditure, over the last eight years, has been insignificant.

March, 1954

8

Tomorrow's Weather

BY FRANCIS BELLO

After Man, after water, comes weather, that perennial topic of conversation. Modern meteorological research would amaze even Ben Franklin, an original, inventive "weatherman" in his own right. But that is not the half of it. Now it is being suggested that global climate will someday be an integral part of future world diplomacy—and war.

IT MAY PROVE EASIER," remarked Irving Langmuir not long ago, "to make the weather than to predict it." However much this remark may irritate orthodox meteorologists, it illuminates the present state of meteorology and its baffling controversies. Ever since Vincent Schaefer made his first dry-ice run over a cloud in November, 1946, meteorology has been going through storms. As is normal in such periods, it is difficult to sort out biases and to separate fact from hypothesis. Since rain has high economic value, many farmers, ranchers, power companies, and others have decided to try the new rainmaking technology without waiting for meteorologists (and statisticians) to agree on its effectiveness. As one businessman ex-

plains, "Cloud seeding is so cheap that you can't afford not to try it." As a consequence, rainmaking has swiftly grown into a multimillion-dollar-a-year business that is spreading around the world.

As Dr. Langmuir's remark suggests, cloud seeding may be more than a local rainmaking stunt; conceivably it may trigger off large-scale atmospheric effects. At least this is his deduction from a remarkable twenty-month experiment which he asserts proved beyond doubt that periodic rainfall over half the U.S. was induced by silver iodide released periodically in New Mexico. The conclusion Langmuir draws is not that silver iodide is so powerful, but that the atmosphere is so unstable, so susceptible to faint proddings, that meteorologists can never hope to predict its behavior with certainty.

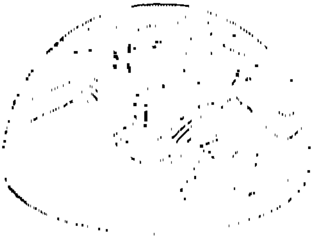
This most meteorologists are reluctant to concede. They are trying to improve forecasting techniques as if cloud seeding had never been discovered. While confident of doing better, they freely admit that present forecasts deteriorate rapidly beyond seventy-two hours, and that the Weather Bureau's experimental thirty-day extended forecasts, on balance, give results only somewhat better than climatic probability allows. On the other hand, the most successful industrial meteorologist in the country, Irving P. Krick, claims to have a sound method for predicting weather three and even six months in advance.

Despite Krick, man's hope of predicting the behavior of the immense and restless atmosphere seems at least as chimerical as his attempt to define the limits of the universe. Yet the effort is going forward in many university and government laboratories, and, as in nuclear physics, the cost is being borne almost solely by the government. With plenty of money available, meteorologists are bringing some of the most advanced tools of modern technology to bear on their problems. They are probing the mechanism of storms with radar, plotting solar activity with coronagraphs, shooting instrument-carrying rockets (one or two a month) to the top of

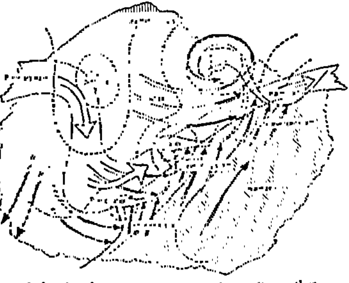
the atmosphere, and solving some of the most difficult equations known to mathematics with electronic computers. At the University of Chicago a young meteorologist has created a remarkable model of the atmosphere in a rotating dishpan. And at several cloud-breeding areas a private foundation, Munitalp (Platinum spelled backward), has installed movie cameras that, by time-lapse technique, make condensed records of cloud activity. Viewing these breath-taking, accelerated movies in color, one has the impression of seeing cloud behavior for the first time.

Yet the atmosphere cannot be examined in microcosm—despite the dishpan. Science must examine in detail and for long periods everything that may conceivably influence the weather, from variations in solar radiation to the temperature and circulation patterns of the oceans. "On some of our problems," says H. E. Landsberg, the Air Force's director of geophysics and weather research, "fifty years will not be too long to work."

Despite their complexity in detail, the basic motions of the atmosphere have a fairly simple origin. At the equator the earth's surface receives much more solar energy than it does at the poles. If there were no atmosphere, equator and poles would reach a temperature based solely on the rate at which they could radiate heat into space. The equator would be substantially hotter and the poles much colder than at present. The atmosphere, in obedience to the second law of thermodynamics, dutifully carries heat from hot regions to the colder. The currents generated by this process are deflected by the rotation of the earth, hence the prevailing winds at low altitudes and the so-called planetary waves at high altitudes. In its effort to reach dynamic equilibrium, the atmosphere is governed by self-regulating mechanisms, i.e., it has, in current jargon, various feed-back controls that provide a basic pattern of regularity, within which an infinite variety of small-scale adjustments may take place. These adjustments are the local weather.



Around the globe: heat, cold, and rotation set up giant waves.

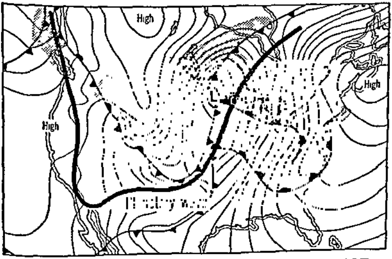


Under the planetary wave, the earth gets its weather.

Weather Rides a Wave

Only in the last fifteen years has meteorology discerned worldwide order in the complex disorder reflected in the familiar sea-level Weather Bureau map. The new order emerged from hemisphere-wide charting of the upper atmosphere, speeded by World War II. Meteorologists found that, between 20,000 and 40,000 feet, wind currents follow wave-like paths around the earth (upper diagram, opposite page). The paths, or planetary waves, provide worldwide "teleconnections" in atmospheric behavior. The waves characterize the motions of a fluid that is heated, cooled, and rotated in a certain way.

Under the waves, at the earth's surface, warm air and cold air tend to react most violently on the upcurve, producing storms, as shown schematically in the lower diagram, opposite page, and on the conventional weather map, below. (The planetary wave was not on the original sea-level map, but was derived from an upper-air chart for the same day.)



Certainty vs. uncertainty

There are two ways of looking at these grand atmospheric processes. The determinists hold that the atmosphere is a fluid that must conform to well-defined hydrodynamic equations of motion. This school owes much to the prodigious work of the Swedish-American meteorologist, Carl-Gustaf Rossby, director of the Stockholm meteorological institute, who established the meteorology departments at M.I.T. and the University of Chicago and taught in the U.S. from 1928 to 1946.

The other, and so far minority, school holds that the atmosphere is always on the verge of instability and that therefore weather is essentially probabilistic rather than deterministic. Irving Langmuir, the leader of this school, finds confirmation for his thesis in cloud seeding. In fact, he holds that cloud seeding is the most useful technique for removing the uncertainty from weather. Langmuir argues that precipitation is not an incidental but a fundamental part of the worldwide weather machine and that heat given off when water vapor condenses and freezes is great enough to influence significantly the motions of the atmosphere. Uncertainty enters, says Langmuir, primarily because water in clouds does not freeze spontaneously, as one might think, at 32°F., and in the temperate regions of the world precipitation seldom occurs until water inside a cloud does freeze.

The next question is: what induces water to freeze? It was while searching for the answer to this problem that Langmuir's General Electric associate, Vincent J. Schaefer, discovered that seeding a cloud with dry ice would turn the trick. Reason: it locally lowers cloud temperatures below −38°F., whereupon freezing appears to take place spontaneously. A little later another G.E. scientist, Bernard Vonnegut, discovered that particles of silver iodide would induce freezing, provided the temperature of the cloud was below 25°F. (General Electric holds the patents on both dry-ice and silver

iodide seeding but has waived royalties.) It appears that natural nucleating agents, resembling silver iodide, customarily trigger the freezing that creates natural rain. These nuclei, however, seldom cause ice to form above 10°F. and often do not work until clouds reach −15° or below. Thus there is a critical temperature gap of at least 15 degrees in which silver iodide will cause precipitation and natural nuclei won't. Moreover, most scientists now agree with Langmuir and Schaefer that the atmosphere is frequently deficient in freezing nuclei.

Langmuir thus concludes that natural rainfall is prone to uncertainty, and that there are in weather, as "in atomic physics, as well as human affairs . . . what we may call divergent phenomena where large important events grow from small beginnings . . ." Even Professor Rossby, the determinist, does not dismiss the Langmuir uncertainty thesis out of hand. "If Langmuir is right," says he, "we are wrong, and our attempts to forecast the weather will fail."

After more than six years of experimentation, meteorologists still cannot agree on cloud seeding's economic value. Moreover, the controversy has passed so completely into the stratosphere of higher statistics that it may be years before any agreement is possible. What makes the issue complex is this: if you seed a cloud and it rains, how can you be sure it wouldn't have rained anyway? Obviously there may never be proof enough to convince a complete skeptic. As one eminent statistician observes, "The more cautious we desire to be in asserting the effect of seeding, the less chance we shall have of detecting such effects if they actually exist."

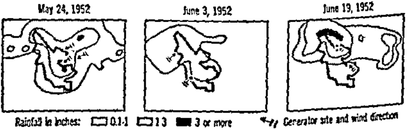
There is no doubt that most of the first weather-modification experiments, including the early tests of Project Cirrus (a joint effort of General Electric, the Signal Corps, and the Office of Naval Research), were conducted without proper regard for the nasty questions that the statistician can pose. Any casual reader of the 1951 Project Cirrus report would conclude that cloud seeding

clearly can produce gigantic effects. It mentions, for example, one seeding experiment in New Mexico that produced about 160 billion gallons of rain, and another that produced twice as much. (New York City's reservoirs hold about 350 billion gallons.) Dr. Langmuir calculated that there was only 1 chance in 100 million that these two rains were natural occurrences. Weather Bureau and university meteorologists found his statistics altogether unconvincing.

Rain for the Sugar of Cuba

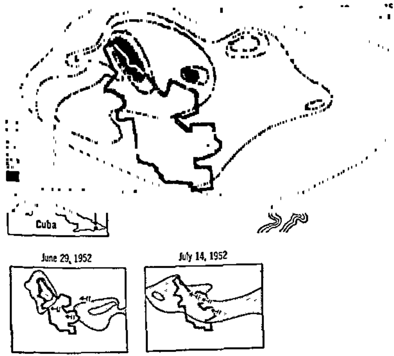
In 1952 thirteen Cuban sugar mills employed the rainmaking services of W. E. Howell Associates. The map at right indicates per cent of normal rainfall that landed on one client's crops during the 1952 cloud-seeding period. The small maps represent five seedings picked at random from the main summer series of thirty-four. Note how rain tends to concentrate downwind from the silver iodide generators. (Rain missed the target on only six seeding occasions.) In the previous eight years on record, rain on the target area was never more than 22 per cent heavier than rain on the surrounding "control" area. In 1952 it was 53 per cent heavier. Howell figures the probability of this occurring naturally is less than 1 in 1,000.

The Odds (over 1,000 to 1)
Say This Pattern Is Man-made →



Rain at the flip of a coin

An independent series of cloud-seeding experiments conducted by the Weather Bureau and the Air Force during 1948 and 1949 gave astonishingly negative results. The results were so negative, indeed, that impartial scientists outside the controversy speculated that the Weather Bureau's experimenters might have succumbed,



quite unconsciously, to bias. In any event, the results left statisticians almost as dissatisfied as did those obtained by Project Cirrus, so the Weather Bureau launched, in the state of Washington, a second series of experiments scrupulously designed in advance to meet all statistical objections. The basic principle observed is randomization. When observers spot a likely-looking cloud, they leave the decision to seed or not to seed to the flip of a coin, or to a list of random numbers. Rainfall under both seeded and unseeded clouds is then compared. The tests are being conducted over two regions that historically receive almost exactly the same amount of rainfall. Any pronounced increase in rainfall over one region will have to be attributed to seeding.

Since the Weather Bureau planned to conduct cloud-seeding tests only from the air, professional cloud seeders protested that the experiments would not test prevailing commercial techniques. They argued that ground-based silver iodide generators must be operated throughout an entire storm and many hours per month to achieve ponderable increases in rainfall, and that brief seeding runs over a few clouds would prove virtually nothing. It would seem that the Weather Bureau might have speeded resolution of the controversy by copying commercial methods, which are based, after all, on substantial experience.

One other noteworthy series of cloud-seeding experiments was conducted in Australia by E. G. Bowen, a member of the Commonwealth Scientific and Industrial Research Organization. Both Langmuir and the Weather Bureau have derived comfort from Bowen's results. On the one hand, Bowen leaves no doubt that dry ice will produce "substantial amounts of precipitation which would not otherwise have fallen, provided conditions are right." * (No comparable statement can be found in the Weather Bureau's first

* *In Bowen's report this statement also applies to the seeding of warm cumulus clouds with water drops, using a variation of a method first proposed by Irving Langmuir.*

series of reports.) On the other hand, Bowen seems to have had little success with a ground-based silver iodide generator. He concluded that in Australia, at least, airborne seeding showed more promise and that it might accomplish a 5 or 10 per cent increase in annual rainfall, "at the very outside." Since airborne seeding costs much more than ground-based seeding, Bowen doubts that rainmaking is of economic value except in marginal-rainfall areas, where a little extra rain at the right time might greatly stimulate the growth of crops. He cites, for example, one Australian wheat area where every additional inch of rain in August and September would increase harvest value over $2 million.

Faced with the Langmuir–Weather Bureau split at home, many uncommitted meteorologists prefer to take their cue from Bowen. Thus, Henry Houghton, chairman of the American Meteorological Society's committee on weather modification, says that Bowen's 5 or 10 per cent figure represents his own estimate of the maximum value of seeding, though he says he has not seen even this figure confirmed by careful experiment. "If seeding produced more than that," says Houghton, "it shouldn't be so hard to find."

Is optimism unethical?

In the light of the foregoing, what is the businessman, farmer, or rancher to conclude about rainmaking? Is it, as Houghton suggests, almost wholly unproved and therefore a waste of money? The commercial cloud seeders feel that the furor over statistical validation has obscured the important fact that cloud seeding works. It works because the atmosphere is frequently deficient (the exact degree of frequency is arguable) in freezing nuclei; and, more to the point, natural nuclei are almost certainly less effective than silver iodide. The average businessman presented with comparable observations from his own laboratory would conclude that optimism was in order. For some curious reason most meteorologists have refused to be optimistic.

The commercial cloud seeders have been criticized by their academic friends for accepting money for applying an immature technology. The seeders retort that farmers and businessmen were anxious to try rainmaking and that if competent meteorologists did not accept the job, others less competent would. The seeders also point out that the technology could not be developed in the laboratory, that it had to be done out of doors, and that neither government nor university scientists seemed interested in doing the job. If one discounts some initial overenthusiasm, it appears that the leading commercial rainmakers have been careful to acquaint clients with the uncertainties involved.

The seeders acknowledge that statistically significant results cannot be obtained in one or two seasons of operation over one target. Sometimes, as in the Cuban operations charted on pages 130–131, the results seem tantamount to documentary evidence. In other cases the results are less striking. All the seeders agree, however, that results have been generally encouraging and that the sum of all their efforts is a strong plus. Their conclusion: *cloud seeding can usually increase precipitation 20 to 40 per cent.* They know as well as anyone that if this belief is not borne out during the next several years their business is washed up.

Pennies from heaven

There are four large cloud-seeding organizations, all run by able meteorologists: Water Resources Development Corp. of Denver (Irving Krick); North American Weather Consultants, Pasadena; W. E. Howell Associates, Inc., Cambridge, Massachusetts; Weather Modification Co., Redlands, California.

Krick's organization, the first in the business (1950), is the biggest. Krick says that his rainmaking sales topped $1 million in each of his first two years. (The usual cost to the client is 1 to 3 cents an acre.) Most of Krick's customers have been farmers and ranchers, but he has also worked for utilities, and in fall, 1952, began seeding

in Spain in a five-year effort to increase water supplies for hydro power. The Spanish contract is with Airfleets, Inc., one of Floyd Odlum's ventures, which will earn fees from several power companies if the project succeeds.

In the summer of 1952 Krick had several hundred silver iodide generators spotted around the western half of the U.S. Usually they were turned on, at a phone call from headquarters, by a farmer or filling-station operator. Krick's procedure, followed in broad outline by others, is to watch the weather closely and, when he sees a storm brewing, to start seeding, using those generators located upwind from the target. "We work in step with nature," explains Krick, "and provide a catalyst to increase rainfall." As for the charge that he and others may be robbing Peter to pay Paul, Krick says nonsense. Nature, he maintains, has difficulty in precipitating even 1 per cent of the moisture available in the atmosphere during storms, hence there is plenty for everyone, even if silver iodide should double the rainfall in selected areas.

One of Krick's most impressive efforts: in the Rockies, where he has been seeding to increase the snow pack (to provide water for Denver), he reports that on target the three-year average snow pack is 175 to 288 per cent of the previous ten-year average.

Rain every Tuesday

Since the professional rainmaker wants to produce local results for a specific client he has been anxious to disclaim responsibility for floods that may occur, coincidentally he hopes, hundreds of miles downwind from his generators. Thus even the rainmakers have not enthusiastically accepted evidence that over a twenty-month period Project Cirrus induced various periodicities in weather behavior over more than half the U.S. by releasing silver iodide at Socorro, New Mexico. It would be difficult to think of another experiment in modern science that seemed to prove so much, and yet was dismissed by the opposition as proving absolutely nothing.

Langmuir conceived his grand experiment, as part of Project Cirrus, after the Weather Bureau and other critics refused to credit seeding with the release of 480 billion gallons of rain on New Mexico in two days. Langmuir figured that if the weather were as capricious as his critics suggested, he would try to make it behave periodically by releasing, every week, about 1,000 grams of silver iodide. Initially Langmuir planned to run the Cirrus generator every Tuesday, Wednesday, and Thursday. The experiment had barely started in December, 1949, when heavy rains, concentrated chiefly on Mondays and Tuesdays, began drenching the Ohio Valley, causing near floods (see diagram opposite). Langmuir cut generator operation to Tuesday and Wednesday. Still the rains came. By April, 1950, the rainfall over the entire eastern half of the U.S. had assumed a striking weekly periodicity. Every Tuesday it rained from Alabama to Minnesota. One or two days later the storms had usually marched to New York and the New England coast. In subsequent months the periodicity became sporadic—upset, according to Langmuir, by commercial cloud seeders who had begun work in earnest. Nevertheless, an analysis by the Weather Bureau confirms that a strong tendency to periodic rainfall existed for the entire eleven months of weekly seedings.

In October, Langmuir began the second part of the experiment in which Project Cirrus tried to set up a weekly periodicity and then shift its phase. The plan called for three generators to seed on Mondays for eight weeks, then shift to Fridays for eight weeks, then back to Mondays, and so on. This experiment lasted until mid-1951. If Langmuir could show that weather in the East followed the phase shifts of the generators, there would be virtually incontrovertible evidence of the power of silver iodide to modify weather.

Did nature obey man?

Langmuir, who has still not published all his data, finds that the phase shift actually took place. Perhaps the only man who has tried

to follow Langmuir's complex statistical analyses in detail is Glenn W. Brier, chief statistician of the Weather Bureau, for whom Langmuir has high regard.

On the crucial matter of rainfall behavior during the phase-shifting part of the experiment, Brier and Langmuir agree on obser-

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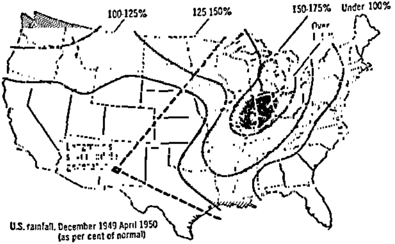
Weather under Control?

One of the strangest and most controversial experiments in the history of science was conducted over an eighty-four-week period by Irving Langmuir, General Electric's distinguished Nobel laureate. As part of Project Cirrus, Langmuir tried to induce periodicities in weather behavior by seeding the atmosphere periodically with silver iodide from a generator in Socorro, New Mexico. As the map indicates, no sooner had Langmuir started his generator running on a weekly cycle than heavy rains began drenching the Ohio Valley—on a weekly cycle. Weather Bureau records showed a striking tendency toward Tuesday rainfall in a belt stretching from Alabama to Minnesota during April, 1950. Periodicity in other eastern states was almost as marked.



U.S. rainfall, December 1949-April 1950
(as per cent of normal)

vations, disagree on significance. They agree that some phase shifting appeared to take place in the vicinity of Georgia, Alabama, Florida, and Tennessee, but not anywhere else. Brier maintains that the widespread absence of a phase shift makes its appearance in one region statistically insignificant. Langmuir says not so. He argues that many generators were operating north of his during the period, hence he could hardly influence the weather except in the Southeast.

Langmuir naturally feels that no one could hope to obtain more striking confirmation of a hypothesis than he obtained in his twenty-month experiment. Many meteorologists take the view that whatever happened, it was pure coincidence. Others, like Henry Houghton, frankly don't know what to think. "It's the most mysterious thing I have ever run up against," says Houghton. "How could one lonesome generator in New Mexico have the effect Langmuir says it had? If it wasn't chance, it was a totally new effect." Irving Krick's opinion is that the weekly seedings may have amplified and extended a natural tendency of U.S. weather to follow a five-to-seven-day cycle.

"The whole experiment was a great tragedy," observes one meteorologist. "If Langmuir actually influenced the weather, no one will believe him. If the periodicities were mere coincidence, nature played Langmuir a dirty trick."

Electronic prediction

The real test of Langmuir's remark, that it may be easier to make the weather than to predict it, may come not from cloud seeding but from straightforward efforts to predict the weather. The most ambitious forecasting effort is being conducted with the aid of an electronic computer at the Institute for Advanced Study, under mathematician John von Neumann.

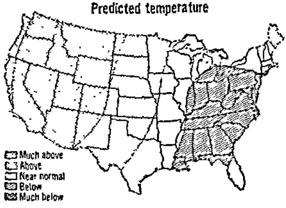
Prediction by computer is called numerical prediction and is strictly a determinist approach. It assumes that the atmosphere, as

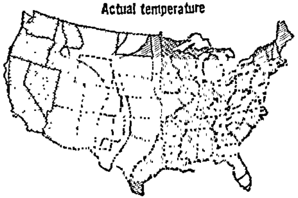
a fluid, is subject to dynamic laws that can be expressed in a solvable series of differential equations. The information fed into the institute computer consists of atmospheric pressure-level readings at 300-kilometer intervals on a grid covering most of the U.S. and part of Canada. (The readings actually represent various altitudes at which a given atmospheric pressure is observed. For example, the 700-millibar level usually lies between 9,500 and 10,500 feet.) Early computations took account of only one pressure level, represented by 361 readings. When the computer was fed 722 readings, representing two levels, the accuracy of prediction improved noticeably. The trick has been to develop a series of equations that will take observed readings and predict the readings that may be expected one hour hence, two hours hence, and so on. For the present, the goal is a good prediction twenty-four hours hence. (For one pressure level this takes the computer six minutes.) It should be stressed that the machine's product requires a good deal of interpretation before it resembles a conventional forecast of weather, but von Neumann sees no reason why, in time, the machine could not be made to give rainfall and temperature predictions in usable form.

The first man to examine the possibilities of numerical prediction was a British mathematician, Lewis F. Richardson, who worked out his equations while he was a stretcher-bearer in the First World War. Electronic computers had not been invented and Richardson estimated that it would take 64,000 people using ordinary desk calculators to keep pace with the weather around the globe. Although Richardson had great vision, the equations he developed did not produce satisfactory forecasts.

When von Neumann, shortly after World War II, obtained government backing to build an electronic computer, he decided that one of the problems he would like to tackle was numerical prediction. Jule Charney, a gifted young meteorologist-mathematician who had worked with Rossby, was brought in to take up where Richardson had left off. Since 1948, Charney has been refining mathe-

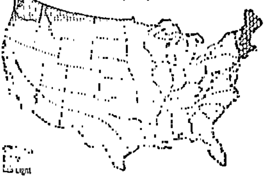
October, 1952:
The Weather Bureau
Was on the Nose





Businessmen and farmers who based their weather-sensitive plans for October, 1952, on the Weather Bureau's thirty-day forecast presumably made smarter moves than those who operated without such aid. The forecast was excellent. Temperatures were sharply split into two great zones. Precipitation was widely subnormal.

Predicted precipitation



Actual precipitation

ence on the path of storm tracks crossing North America. (For other parts of the world there are other reference cells.) Krick, moreover, finds that the high moves about the Pacific in a reasonably predictable manner, thereby providing a basis for long-range forecasts.

Under a planetary wave

Until Krick chooses to publish his methods in detail, meteorologists will have a hard time appraising the validity of his approach. Meanwhile the prevailing body of opinion favors the type of long-range forecasting practiced by Jerome Namias, head of the Weather Bureau's Extended Forecast Section. The section grew out of a government-sponsored effort conducted at M.I.T. during the Thirties by Professor Rossby. Namias, who worked under Rossby, transferred the project to the Weather Bureau in 1940. M.I.T. still continues to do background research.

The basic concept developed by Rossby and his group is that the upper atmosphere contains planetary waves—sinusoidal air currents that girdle the globe and provide the broad-scale setting for local weather. Rossby developed a theory making it possible to predict the general movement of the planetary waves, which, in turn, provided a rational basis for predicting the weather underneath. While the method cannot forecast the day-to-day weather, it permits one to find areas where temperature and rainfall may deviate from normal. Namias' section makes such predictions for five- and thirty day periods. (*A year's subscription to the thirty-day forecast* costs $4.80.)

During World War II Namias' five-day forecasts were compared with those made by Krick and by C. L. Mitchell, a top-notch Weather Bureau forecaster who had a system based largely on his long experience. (At that time only Krick had a thirty-day method.) The evaluation group concluded that there was, at that time, *little* to choose among the three methods.

Where the jet winds blow

Until that remote day when thirty-day forecasts are made by electronic computer, improvements in present methods may come from several directions. One hope is that better knowledge of the jet stream will make it easier to predict planetary-wave behavior. The jet stream is a fast-moving (up to 300 miles per hour) river of air that seems to define the core of planetary waves, 30,000 to 40,000 feet above the earth.

A recent observation made by Vincent Schaefer may simplify the task of keeping track of the jet stream's location. In studying the cloud movies made by Munitalp, of which he is scientific adviser, Schaefer concluded that about 80 per cent of the time the jet is accompanied by several telltale cloud types. The Air Force and commercial airlines hope they can use the jet stream to speed their aircraft, and Schaefer's clouds should help put them on its track.

Another extremely promising avenue of research, which could help both Namias and Charney, is the weather-in-a-dishpan experiments of Dave Fultz of the University of Chicago. His swirling liquids not only create patterns strikingly like planetary waves, they also generate thin swift currents that resemble the jet stream.

What's the paper say?

For the hard-pressed daily forecaster, trying to make his deadline, the feverish postwar research effort has not yet produced anything as helpful as the famous Norwegian polar-front theory, now some thirty years old. While the Weather Bureau's twenty-four-hour forecasts are approximately correct 85 per cent of the time, a substantial part of this score is built up during periods of easily predicted weather. When sudden weather shifts occur, the forecasters are frequently caught off base.

If Ben Franklin, who is credited with being the first to appreciate that weather moves across the map, were forced to judge modern

meteorology solely by the daily weather forecast, he might be dismayed at the lack of progress. If, however, Franklin could glimpse the rockets, the radar screens, the radiosonde balloons, Jule Charney's computer, and Dave Fultz's dishpan, his dismay might disappear. As a born experimenter he would surely recognize Langmuir and Schaefer as kindred spirits. And it would be difficult to conceive of his being pessimistic about the long-term prospects of weather modification.

May, 1953

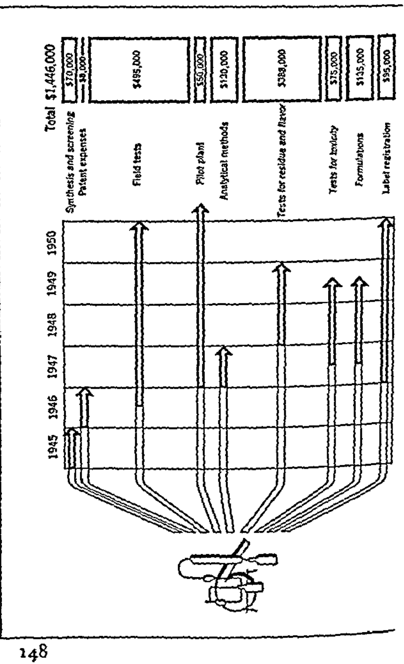
9

Farming's Chemical Age

BY ERIC HODGINS

While U.S. meteorologists chart the clouds and wind-drifts, another band of researchers in American technology are getting down to earth: the agrichemists. Thanks to them, U.S. agriculture is now in the midst of its greatest change since tractors supplanted the horse. Here is how science can at least double our farm productivity in the next two decades.

U.S. AGRICULTURE REMAINS firmly on the highest production plateau in its history. The tonnages of product, edible and inedible, coming from the U.S. farm continue to tower above those of the prewar years—yet these tonnages are being achieved by ever fewer man-hours spent in farming, and from an acreage that has increased in no significant way since 1920. Although we are close to the limit of land on the continental U.S. that can be placed in cultivation, each year helps further to establish that U.S. farm productivity is nowhere near its upper limit; that it can, in fact, be at least doubled in the next two decades. Such an increase can come about through


1945
1946
1947
1948
1949
1950
Total $1,446,000
Synthesis and screening
$70,000
Patent expenses
$8,000
Field tests
$495,000
Pilot plant
$50,000
Analytical methods
$120,000
Tests for residue and flavor
$388,000
Tests for toxicity
$75,000
Formulations
$135,000
Label registration
$95,000

The Prenatal History of an Insecticide

"Three years and a million dollars" is the rule of thumb by which the [illegible]

Synthesis and screening take place in the company's laboratory, first to create a new molecule, then to determine its killing powers. *Patent expenses* are mostly legal fees involved in conducting the search to establish priority. *Field tests* cost most in dollars. Once past the laboratory screening, UGH had to be tested under realistic outdoor conditions. Much of this was done by land grant colleges and by the various field stations of the U.S. Department of Agriculture, via grants-in-aid made by the hopeful manufacturer. First field tests on UGH, as with many insecticides, were run on cotton because it represents (*a*) a huge market, and (*b*) a nonedible crop. *Pilot plant:* After UGH was successful on nonfood crops, the manufacturer set up enough capacity to produce 10,000 pounds for limited use on temporary certification from U.S.D.A. that it looked like a reasonably safe bet. *Analytical methods:* The setting of strict quantitative procedures that will later indicate the amounts of UGH left on the crop sprayed, after a given time. *Tests for residue and flavor:* [illegible]

in what organs does it deposit? From this, what is to be inferred about its effect upon man? *Formulations:* Now that the active ingredient of UGH has proved itself, how should it be put up into a formula that [illegible]

new-model television set.

the persistent use of materials and techniques now known and proved, which today give the farmer a degree of control over nature unknown until a few years ago.

In reaching his present degree of control, the farmer has been considerably helped by an industry that is also striving, in many enthusiastic ways, to put him out of business: the chemical industry. By synthesis the chemical industry now spins out each year increasing tons of fibers that once could be produced only on the farm. It is constantly threatening to produce food: something edible, if not appetizing, out of cellulose. Meanwhile, however, it also acts as shield and buckler for the American farmer in two major ways. One is by the production of greater and greater quantities of fertilizer. The other is through a complex producer-consumer relationship that is now providing some five hundred or more new organic chemical compounds for use in the farmer's fields and barns—to increase his yields, to lessen his labor, to enhance the quality and diversity of his crops, and to rid him of a series of hazards that, through the centuries, he has come to think of as God-ordained.

"It is quite possible," says Charles R. Sayre, boss of Delta & Pine Land in Mississippi, and thus head of one of the largest cotton plantations in the world, "that chemicals for selective control of plants could mean even more to the future of American agriculture than the shift from horses and mules to tractor power." These are strong words. Most of the chemicals Charles Sayre is talking about did not exist on the market ten years ago. They now make up such a profusion of herbicides, soil conditioners, fungicides, defoliants, growth regulators, and—last but greatest—insecticides as would leave a French peasant struck dumb on his manure pile. In 1952 the U.S. farmer, aided and guided by the U.S. Department of Agriculture, the various state extension services, and by the county agent, bought between $350 million and $400 million worth of these products, thus devoting to them about 1 per cent of all farm

income. He also bought close to 100,000 power machines to apply them.

Into the new manufacture and marketing of specialty chemicals for agriculture almost every chemical company in the U.S. has leaped. The giants like du Pont, Union Carbide, Allied Chemical, Monsanto, and Dow are active suppliers to this market. The great petroleum refiners are there, represented particularly by Shell and Standard Oil of California. Because many of the new chemicals in agriculture are chlorinated hydrocarbons, the makers of chlorine—Mathieson, Diamond Alkali, Pennsylvania Salt, *et al.*—are pressing hard for their share of sales. For strange but chemically sound reasons, rubber companies are represented too, principally by Goodrich and U.S. Rubber. In 1934 there was newly formed a trade group now called the National Agricultural Chemicals Association, with fourteen members; in that year the industry produced 100 million pounds of stuff. Today a membership of 140 pours out a poundage that has been estimated—there are few firm figures in this area—as high as two billion.

The new organic world

The new impact of chemistry on agriculture is changing a way of life profoundly. On large farms it has made the airplane, for spraying or dusting or for applying fertilizer, an almost essential tool. Power, and more power, is needed to operate heavier ground machinery. The shelves of the local feed store, or of the farmer's co-operative supply, are taking on more and more the aspect of a Walgreen Drug Store in Times Square; the changes in merchandising pressures and farming practices are as thoroughgoing as the changes in chemistry. One distributor to the trade (there are hundreds of formulators and blenders for every manufacturer) takes an ad for his product to say to his dealers, "The right odor becomes an increasingly important sales factor; with many purchasers, odor is the *dominating* sales factor." But along with this kind of talk there also

goes a new scientific seriousness, and the farmer is cautioned, on the label of an insecticide: "*Droplets smaller than thirty microns do not deposit well on foliage or insects.*"

So bewildering has the diversity of products become that, as the volume of inquiry from farmers to county agents grows too big to handle, private consultants enter the field to prescribe for a farmer's ailing or beleaguered crops as a veterinary now prescribes for the sick livestock. The American farmer lost $5 billion worth of crop to weeds in 1952. He lost $4 billion to insects. He lost another $4 billion to plant diseases. To rats and a final miscellany of misfortune he lost enough to bring the grand total up to $15 billion. If that much extra crop had been for sale it would not, of course, have brought that price; in fact, any such burst of plenty would terrify the politicians of agriculture. But the $15 billion is a good measure of depredation that, says a knowledgeable journal of the American Chemical Society, "could be prevented by the effective application of existing knowledge." It comes to 40 per cent of "the potential salable portion of the farmer's crop each year."

What is happening now is that news, excitement, research, accomplishment, and high sales pressure have drenched a field that used to be singularly dry and dull. Specially synthesized and rigorously screened organic chemicals, tailor-made to specific measures, are moving in to perform enormous new, laborsaving tasks. They have cut, in half or more, the tonnages of "old line" products in the field—the arsenicals, the lime-sulfur mixtures, the nicotine sprays, and pyrethrum dusts—which were largely empirical, "cookbook" formulations that had to be used in big, bulky quantities to perform their limited tasks. The new organics—"the phenoxys," "the di-nitros," "the carbamates," "the organic phosphorus compounds," *et al.*—move with precision and minimum fuss to fulfill highly specified needs. An ounce of a carbamate can protect enough seed for ten acres; a gallon of a phenoxyacetic acid ester can kill more weeds than seven men with seven hoes could kill in seven years. This sort of chemical change not only makes possible a great

improvement and further increases in productivity on well-managed farms but renders economic the clearing of land valued at as little as $20 an acre. Herein a new agrichemical revolution lies—here, or hereabouts.

As with all progress, it does not provide an unmixed blessing. The new insecticides and fungicides are highly toxic, and they persist; that is why they are good. It is also why they are dangerous if used with negligence or in ignorance of some of their subtle effects. In the field, unforeseen conjunctions of heat and rainfall, after a well-tested weed killer has been carefully applied, can turn—and have—an intended precaution into an unpremeditated disaster. But the assets far outweigh the liabilities, present and potential, and to those more interested in volume of production than in the support of prices or the preservation of obsolete menial tasks they strongly suggest that the dawn of farming's chemical age will carry a hope and significance beyond the shores of the U.S., far into a world that does not get enough to eat.

The joyous, gloomy industry

The agricultural-chemicals industry is full of gleeful chemists and doleful sales managers. The chemists are gleeful because their fields of inquiry are booming and blooming so mightily. "We're just never going to run out of problems," said one of them joyfully.

This solid truth, so acceptable to the chemists, is also the precise cause for the dolor of the sales managers. They are trying to keep control over what has been, in its few short years of life, a manic-depressive business. The business burst into a manic phase, with terrific demand and a throttling shortage of basic supply chemicals, after World War II, and led a brief high-profit life that attracted a host of companies into what looked like exciting and nourishing competition. It was competition, all right. The price of DDT, the product that dramatized the industry into its new being, broke from an original 1945 figure of $1.60 a pound to around 30 cents in 1948, and a number of marginal or high-cost companies

were shaken out of the business. There then ensued, 1949–51, three of the most terrific pest years in the history of infestation, and the industry resumed its posture of high prices, strong demand, and short supplies. Staid, conservative companies built new plants to turn out hosts of the new products, and the industry leaned back to await the golden deluge. But the industry's history for 1952 can be written in three tragic words: not enough bugs. In 1953 there were more bugs, but profits remained slim.

Trial Balance Sheet for Farm Chemicals

ASSETS

1 Enormous crop losses from insects, weeds, and disease are becoming more and more avoidable.

2 Large acreages of submarginal land can now be upgraded by cheap chemical killing of brush.

3 Bigger crops can be raised from the same amount of water and nutrients.

4 Quality and variety of crops can increase along with yields.

5 Much less labor (but more capital, in terms of equipment and supplies) will be needed to grow equivalent crops.

LIABILITIES

1 The bug-eat-bug balance of nature can be seriously upset; pollination is endangered by the unintended killing of bees and butterflies.

2 Subtle health hazards to humans must be incessantly watched; effects on food flavors can also be adverse.

3 Farm management, supervision, and labor must cultivate a new and unaccustomed care and precision in their work.

4 Short-range, crop surpluses can cause political troubles.

5 The possible disemployment of as many as 1,500,000 farm laborers could also produce political friction in hard times.

"The farmer is certainly a capitalist," said one sales manager recently, in tones that could have been misunderstood in Moscow. "Over and over, I have to demonstrate to him that by spending a dime he can make or save a dollar. Anything lower than a one-to-ten ratio and it's no sale." He glanced gloomily out the window. "This is a very discouraging business," he intoned. "What with the conservatism of the farmer, the vagaries of nature, the unpredictability of bugs, the lousy competition, and what we have to go through with the Food and Drug Administration in Washington on toxicity standards, I don't know why I'm in this line of work." He took a deep breath. "There are also," he added in the manner of a man overborne by disaster, "our own God-damned research departments that are all the time coming up with something new and better to obsolete a product just as we've got it halfway established on the market."

Missions accomplished

The happy chemists in the research departments (plus wide varieties of biologists, entomologists, plant physiologists, agronomists, etc.) are doing just that. It is a slow year in which the industry screens fewer than 10,000 different compounds to discover what will kill what, and how, and the agrichemical research bill has now climbed to something like $10 million a year. As this practical and profit-directed research grows in volume, there also grows the feeling that only the vaguest principles governing plant growth in health and disease are understood at all, that the soil itself teems with mystery, and that eminently practical worlds exist to conquer, of which no commercial Alexander had any idea before the end of World War II. Even so, the agrichemicals industry, in the brief, confused years of its new life, has already achieved an impressive history. Witness the powers of its new products:

► *Speed.* More than 350,000 acres of rice in California were saved, *in four days*, by airplane spraying of a chlorinated hydrocarbon (dieldrin) to check the attack of a rare leaf miner.

► *Ease.* Against wireworms, four ounces of new insecticide (lindane) have demonstrated that they can do the same job that would have taken one *ton* of naphthalene.

► *Efficiency.* It now costs one-half as much as formerly to control rangeland and cropland pests like grasshoppers; the control efficiency has risen from 60 per cent to 98 per cent.

► *Yield.* The control of greenbugs with the new insecticides has increased wheat yields in Oklahoma by as much as 400 per cent; alfalfa-seed production in Utah has risen 150 per cent; hybrid-corn production increases due to better insect control are estimated at 30 per cent; Maine potato production can be increased 100 per cent.

► *Man-hours.* Requirements for producing a bale of cotton are sinking toward 10 man-hours, from a prechemical high of 155.

► *Money.* The chemical control of livestock pests is now saving the nation as a whole some $800 million a year.

► *Reclamation.* Through the use of chemical weed killers, it may now be possible to convert some 75 million to 90 million acres in the Southwest from mesquite-infested brushlands to a good grade of range.

With the new materials not yet rounding out their first decade, it is obvious that this is only the beginning of farming's chemical age. "The farmer," says Dr. M. T. Goebel, head of research for the Grasselli Chemicals Department of du Pont, "is certainly not spending for disease and pest control any more than 5 per cent of the value of the total potential objective" of stopping his enormous crop losses.

How much he should spend, and how much he will, are two different, important, and still unanswerable questions. The fertilizer industry, in its own promotions to the farmer, is now heavily urging the use of insecticides and weed killers too, for the self-enlightened reason that the farmer won't repeat a fertilizer purchase if he loses a crop to weeds and insects. There is now a good deal of discussion about mixing fertilizers and pesticides together for single applica-

tion. Most agricultural chemists don't like this idea at all, but the learned Dr. George Decker of the Illinois Agricultural Experiment Station takes a realistic view. "Whether we like it or not," he says, "there's going to be a shotgun wedding, and the farmer holds the shotgun. He isn't going to fertilize today and dust next week if he can do the two at once."

Crop	*Yield: 1950–52 (average harvested acre)*	*. . . 1975 (est. per cent increase)*	*Contributory Causes for Increase*
Corn	38.1 bu.	210	Hybridization, close spacing, fertilization, disease and insect control
Cotton	274 lbs.	183	Pre- and post-emergence chemical weed control, improved disease-resistant varieties, greatly improved insect control
Wheat	17.0 bu.	147	Disease-resistant varieties, improved fertilizing and soil practices, more insect and weed control
Potatoes	247 bu.	153	Fertilization, irrigation, improved varieties, disease-free stock, insect control
Tobacco	1,274 lbs.	123	Improved varieties, chemical treatments for better resistance to nematodes and to disease
Pasture and hay	1.41 tons	165	Fertilization, improvement of grasses, mixtures, legumes; improved brush control

The insecticides

The U.S. currently gives asylum to 82,500 different insect species, plus 2,600-odd mites and ticks. Although insects cost the farmer a billion dollars a year less than weeds do, it is against the insects that

he now spends his biggest defense appropriation: better than $200 million last year. There are not as many insecticides as there are insects; it only seems so. Actually, despite a vast confusion of names —which may represent chemical entities, blends, shorthand descriptions, or somebody's trademark—a baker's dozen of workhorses perform the major tasks. The top beneficiaries are cotton, tobacco, vegetables, and fruit—the high cash-per-acre crops. Cotton stands above all. A combination of chemical control of insects and weeds, the defoliation by chemicals, and the spread of mechanical picking is reducing *by more than nine-tenths* the amount of labor required to produce a cotton bale.

DDT begins it

DDT, by stopping cold a typhus epidemic in Naples in 1944, when the U.S. Fifth Army dusted 1,300,000 more or less lousy civilians with it, created one of the most dramatic news stories even of those drama-packed years, and thus set off the modern train of agrichemical events. Yet DDT (short for dichlorodiphenyltrichloroethane) was first synthesized by a methodical German in 1874; its usefulness remained unknown until a Swiss, Dr. Paul Muller, discovered its enormous powers in 1939, whereupon the Swiss Geigy Co. swiftly patented it as an insecticide. It was the first big strike in organic chemicals with a high killing power and a long *residual* effect. Today almost 100 million pounds of DDT are sold annually in the U.S.; battered by this flood, the sales of old-line lead arsenate dropped from 90 million pounds in 1944 to 18 million in 1952.

But as the world now knows, DDT is far from perfect. It failed against the boll weevil, that scourge of the cotton fields that even in 1952 destroyed half a billion dollars' worth of crops. Its long staying power is its strength and its weakness; its use is no longer permitted with dairy cattle because it accumulates in a cow's fat and is likely to show up in her milk. In humans, also, a certain amount of DDT comes to rest in fatty tissues, with possible un-

happy results. DDT, like most of the new organics, is a neurotoxin: it attacks the central nervous system, and is not merely a "stomach poison," like the old-line arsenicals. For that reason it can also play particular hob with the more highly organized insects like bees and butterflies, on which we depend for the essential task of pollination, and this remains a serious problem.

Also, DDT, as the first highly lethal, residual-effect insecticide, gave entomologists a startling look into what happens when the balance of nature in the insect world is drastically upset. For example, the parasites and predators of a pest may be even more affected by DDT than their host for whom it was intended, and insufficient spraying in this case may have the dismaying effect of apparently increasing the infestation. It is this that forces from an occasional layman, driven into the hills with his hand sprayer, the bitter remark, "They've got so they love the stuff."

And the heart of mosquito-ridden America broke a few years ago when it became evident that the subkingdom *Insecta* was fighting back successfully against DDT by genetical mutation. New resistant strains of houseflies, mosquitoes, and potato beetles have bred themselves; houseflies in California can now shrug off a dose of DDT 2,000 times greater than what would have put an earlier ancestor under the table for good.

To kill the bugs

The search for the perfect insecticide thus goes on and on. This is a more or less chronological list of the more effective newcomers that have followed in the boiling wake of DDT:

BHC, or *benzene hexachloride*, which was developed in England and France, and first put on the U.S. market in 1946 by Hooker Electrochemical, now sells almost as much as DDT: 98 million pounds in 1952. It has the great advantage of attacking the cotton boll weevil and some aphids that DDT did not affect. It has, however, a persistent and intensely disagreeable odor, which makes its

use on root crops out of the question. Its so-called "gamma isomer," now on the market as *lindane*, has all the insecticidal potency of technical BHC, and does not stink; this makes possible its use on root crops and such delicacies as melons, strawberries, cucumbers, and asparagus. Its higher cost, compared to BHC, holds sales down to a volume of around two million pounds a year.

In 1946, du Pont marketed *methoxychlor*. It has lower toxicity for warm-blooded animals than other chlorinated hydrocarbons, and thus can be used to rid dairy cows of flies and other annoyances because it does not tend to deposit in the fatty tissues. It can also be used safely on food crops.

Chlordane followed next. It is more volatile than DDT, thus lacking some of the dangers—and advantages—of the highly residual sprays. It represented, as of 1946, the first of a series of insecticides for which the U.S. was in no way beholden to Europe, for it was a chlorinated hydrocarbon synthesized and tested by the brilliant Dr. Julius Hyman, whose smallish company was later bought by Shell Chemical. Chlordane, made by the Velsicol Co., is useful against grasshoppers, army worms, cutworms, roaches, and ants, and, as a soil insect disinfestant, against white grubs, Japanese beetle grubs, etc.

TEPP, short for tetraethylpyrophosphate, owes its existence to the chemical-warfare research of Nazi Germany. It was a product of the German chemist Gerhard Schrader, whose work was discovered by U.S. and English intelligence teams after World War II. American Cyanamid first brought it out as an insecticide in the U.S. in 1946. It is used principally by fruitgrowers against orchard mites and aphids. Although highly toxic, it has almost no residual effect; it all but disappears from the sprayed crop in twenty-four hours, which makes it valuable to the fruit and truck farmer.

Toxaphene was the second all-American insecticide of importance. Hercules Powder brought it out in 1946, a chlorinated product not of the petroleum industry but of the naval stores (turpentine and

rosins) that play a large part in Hercules' life. Its name was originally a Hercules trademark, but Hercules later released it as a generic name. It has had major success against the boll weevil and grasshopper and a wide range of cotton insects, but not against the red spider, a tough customer for any but the most lethal to handle.

Parathion was put on the market by American Cyanamid in 1947, and now enjoys high importance; between five and six million pounds of it were sold in 1952. Irritable words are occasionally exchanged between its makers and users on the one hand, and the Food and Drug Administration on the other, because of its extremely high toxicity. Like TEPP, it is a de-Nazified organic phosphorus product—strictly for professional use, and with no place in the kitchen or garden or near livestock. It is a mainstay of the Florida citrus industry in controlling its bane, Florida red scale, as well as the red spider, aphids, and leaf hoppers.

Since 1950 the flood has scarcely slackened at all. *Aldrin* and *dieldrin*, both chlorinated hydrocarbons developed by Dr. Julius Hyman and manufactured by Shell, are two highly aggressive newcomers in what has come to be one of the most intensely competitive—and overcrowded—bazaars in U.S. business. Aldrin is particularly effective against grasshoppers, whose visitations, a subject of horror since Biblical times, need no longer be feared by any righteous man, provided he hath a power sprayer of sufficient capacity. Aldrin finds a great deal of work to do in the cotton fields, which are sometimes infested with as many as a hundred different insect pests in addition to the boll weevil. It is certified now for use also against a variety of soil insects and half a dozen turf, nursery, and greenhouse pests. Dieldrin is less specific; besides doing many of the things aldrin will do, it can also be used against the chinch bug, several fruit insects, and the so-called public-health insects: houseflies, mosquitoes, chiggers, ticks, and fleas.

Allethrin is another newcomer of high importance: in 1952 commercial production was some 60,000 pounds. It is the product of re-

search in U.S.D.A. itself, and is important because it resembles a synthetically produced pyrethrum, the dried flowers of a plant hitherto imported from British East Africa and the Belgian Congo. Pyrethrum is virtually nontoxic to warm-blooded animals, and is therefore a kitchen boon in the fight against houseflies and mosquitoes.

Next step?

For some time now it has been the dream of the insecticide business to develop a practical *systemic* insecticide: something that, applied to the plant once, would not just lie on its foliage but get into its sap stream, and make every part of it poisonous to any insect feeding on it. This would have two enormous advantages: greatly reduced spraying time would give the plant the fullest protection, and beneficial insects (i.e., those that do not chew or suck plants) would come to no harm from systemics.

Hundreds of would-be systemics are being screened by manufacturers or are under U.S.D.A. scrutiny, but so far in this country only one is certified ready for the agricultural market. This is *Systox*, a complex organic thiophosphate put out by Chemagro Corp. and Pittsburgh Agricultural Chemical, for which the U.S., once again, owes its thanks to Hitler's Gerhard Schrader. The U.S.D.A. permits its use on cotton for the control of aphids and mites. Several other systemics, put out by Dow and Monsanto, have been O.K.'d for greenhouse use, but there is not yet any systemic in sight for food crops. Its arrival, however, would seem to be only a question of time.

The herbicides

The world of weed killers is much less turbulent and confused than the world of insecticides, but even more may come out of it in the long range. For one thing, the farmer's tribute to weeds is conspicuously his greatest, but he does not spend as much on weed control as on other troubles that cost him a quarter as much—

animal diseases, for example. Yet in the opinion of R. L. Lovvorn and W. C. Shaw, agronomists writing with all the authority of Beltsville's Division of Weed Investigations behind them, the farmer's loss to weeds could be halved right now—that is, an economic waste of $2.5 billion a year could be ended any time the farmer is so minded. Weeds demand top priority for water, light, and nutrition—and complaisant nature gives it to them. "The average ragweed plant," say Shaw and Lovvorn, "has a water requirement three times that of corn. One plant of common mustard requires twice as much nitrogen, twice as much phosphorus, four times as much potash, and four times as much water as a well-developed oat plant."

Dr. Francis J. Weiss, author of *Manpower, Chemistry and Agriculture*,* has illustrated what weed control can mean: with *no* control a typical acre may yield 7 bushels of corn in an average season; with complete control a similar acre can yield 53 bushels—more than seven times as much. The laborsaving possibilities of chemical weed control are just as dramatic; in the cotton fields one man with the right chemicals can now do the work that once required the back-breaking labor of a hundred. Last year, says the U.S.D.A., a total of 30 million acres of U.S. cropland were chemically treated to kill weeds. This is impressive—yet it represents less than 10 per cent of our continental cropland total.

This new state of affairs is traceable back to pre-World War II days, when a curious chemical, by name 2,4 dichlorophenoxyacetic acid, came to the attention of Dr. Percy W. Zimmerman, eminent and crotchety plant physiologist at the Boyce Thompson Institute for Plant Research in Yonkers, New York. This chemical interested Dr. Zimmerman because of its qualities as a plant hormone, or growth regulant. Dr. Zimmerman discovered many things 2,4-D would do, some useful and some bizarre, and a number of chemists

* *A remarkable staff report to a U.S. Senate subcommittee, 1952; Senate Document No. 103.*

and plant physiologists enlarged upon this work. In 1943, the year before DDT stopped the typhus epidemic in Naples, du Pont, which had been going it independently, got a patent on the use of 2,4-D as a growth regulant. In that same year a small company, by name American Chemical Paint Co., applied for a patent on 2,4-D, *not as a growth regulant but as a general weed killer:* it could stimulate plants into a morbid adolescence of which they quickly died. This patent was granted in 1945, and soon thereafter 2,4-D began displaying remarkable powers indeed: it would kill dandelion and plantain but not harm a lawn.

Swiftly thereafter it became apparent, to the horror of those, like du Pont, who had been close to Truth but only grazed her brow, that 2,4-D was a truly miraculous substance and that a great principle had been at last confirmed: the principle of the selective destruction of plants. 2,4-D attacked broad-leaves but left grasses alone, and American Chemical Paint's use patent on 2,4-D as a general weed killer retained for it the right to commercialize this quality, discovered by others. The old and indiscriminate plant killers of the past, the iron sulfates, sulfuric acids, and refuse crankcase oils, started on the way to oblivion.

Because 2,4-D is not a poison, it represents only a slight hazard to the animal world. Yet it is certain death to such noxious pests as common and giant ragweed, wild mustard, cocklebur, and bull thistle. This does not mean that it can be used carelessly; since it is a hazard to most broad-leaves, it cannot be permitted to drift by accident over crops like cotton, clover, grapes, soybeans, or tobacco, or over such row-crop delicacies as tomatoes, lettuce, spinach, or broccoli. It has to be kept away from peas and other legumes. Yet it can be sprayed over a suburban lawn, the broadest fields of corn, or over 5,000 acres of wheat with equally good effect.

New techniques

"Control of wild mustard in wheat now seems simple," say the Messrs. Shaw and Lovvorn, "yet ten years ago this accomplishment

was impossible." At present prices, they go on, the chemical cost is 25 cents an acre, and "at present hourly wage rates, the cost of 2,4-D to control mustard in wheat is less than the cost of walking across an acre of wheat, unless the owner goes at a gallop." The new pesticides like 2,4-D have called into being a whole new technique of application. Airplanes, now in steadily increasing use for large operations, can treat a hundred acres via low-gallonage, high-concentration sprays in half an hour of flying; it is possible, although difficult, to make as little as 3 gallons of spray cover an acre. Power sprayers have been developed for on-the-ground work with spray swaths of from 30 to 50 feet.

The so-called phenoxy group of chemicals, to which 2,4-D belongs, have been responsible for another and extremely powerful weed-control technique: the so-called "pre-emergence application." The wanted crop is planted; several days later the bare ground is sprayed in rates of application somewhat higher than for foliage sprays; weed *seedlings*, whether of grasses or broad-leaves, are by this method killed as their first shoots appear, whereas the seeds of the wanted crop are protected by their tolerance and depth of planting. A close chemical cousin to 2,4-D (Crag Herbicide-1, as marketed by Union Carbide & Carbon) does even better: it seems to be completely inert in contact with the foliage of almost all plants, so that there is no drift danger; but when it comes into contact with the soil, microorganisms alter it into a herbicide destructive to seedlings that may be germinating in the topsoil.

The so-called "di-nitro compounds," of which Dow Chemical's Premerge is a top commercial example, also have selective killing properties and also, like the phenoxy family, are becoming intensely valuable for pre-emergent treatments. So is calcium cyanamid. Chemists, biologists, and plant physiologists are somewhat vexed that they have only incomplete knowledge of the exact mechanisms through which their various wonders are performed, but the farmer, who asks only that a dime invested in such things as weed killers will make or save him a round buck, is delighted.

Death to brush

Still another phenoxy compound is assuming constantly larger importance: this one is 2,4,5-trichlorophenoxyacetic acid, or 2,4,5-T, which is death to brush and woody plants but harmless to grasses and to wildlife. It now, and at last, makes possible the destruction of sagebrush and mesquite by nonmechanical means. "If sagebrush," says the Colorado Experiment Station, "can be controlled economically by chemicals, about 4 million of the 7 million acres of sagebrush land in Colorado could produce \$7 worth of beef, lamb, and wool per acre, instead of the present \$2 per acre." In 1953 the U.S. Soil Conservation Service announced that something between 6 million and 25 million acres of scrub forests in the Ozarks could be cleared away, largely by chemical means, for conversion into grazing country: 2,4,5-T would have a major part in that conversion, whenever it is to begin. In the Southwest there are 75 million acres of mesquite-infested lands, and they are a most attractive prospect for the new agrichemicals industry.

But here Dr. J. K. Northway, of the King Ranch in Texas, which is, among other things, the biggest private bush-clearing agency in the world, files a minority report. During a severe and sustained drought, even the hardiest grasses die, and cattle, if sufficiently hard pressed, *will* eat the beans of the mesquite tree. Dr. Northway thinks the chemical boys should go slow in their plans to get rid of it altogether. The King Ranch is rooting hard for the chemical revolution in agriculture but thinks it is "still in its Model T stage."

The ability of 2,4-D to kill broad-leaves and to leave grasses unaffected was no help to the growers of cotton, or to truck farmers, the two groups whose products account for the greatest number of man-hours in agriculture. What these men wanted was the precise reverse of 2,4-D: something that would kill grasses and leave broad-leaved plants alone. They have not yet got exactly what they want, but with the development of isopropyl-N-phenyl carbamate (IPC

for short) and TCA (trichloracetic acid) some of their prayers were answered. A *chlorinated* carbamate, CIPC, is helpful against crab grass in cotton. The annual grasses and weeds are now on the run, but there is still plenty of trouble unsolved from such deep-rooted perennials as bindweed, nut grass, and Johnson grass. TCA also goes after the grasses; it finds a major use in Hawaii, where several million pounds a year are used against grasses in sugar cane.

Defoliants

The subtleties of plant-growth control now possible are not at all confined to selective killing. In cotton, particularly, the rise in mechanical picking set up a demand for something that would cause the leaves to drop off plants, and make it possible for the machine to do a clean picking job. In 1942, American Cyanamid put its calcium cyanamid on the market for this specific purpose. Some 55 million pounds of various defoliant chemicals (calcium cyanamid is good for cotton, but not the only specific) were sold to the farmer in 1952—and in California alone more than half a million acres of crop were sprayed with defoliants from the air. They are needed in soybean harvesting and the production of legume seeds, as well as to kill the tops of potato vines. This last is a case of diamond-cut-diamond. Treated with modern insecticides and fungicides, potato vines grow so luxuriantly that it is now necessary to desiccate them artificially to preserve potato quality.

Growth regulation

Although 2,4-D, originally marketed as a growth regulator, found its major usefulness elsewhere, an appreciable quantity still gets used as first intended. It has now been joined by hosts of other substances, among them maleic hydrazide and various esters of complex acetic acid derivatives, to take on the following tasks:

► To thin out the blossoms on fruit trees and thus produce a better set of fruit without handwork

► To delay fruit budding until the danger of late frosts is past
► To prevent fruit from dropping
► To produce fruits without seeds
► To halt flowering where not wanted, as with vegetables like spinach, broccoli, etc.
► To alter the shape of plants for easier harvesting
► To prevent sprouting in stored potatoes or grains
► To slow the growth of grass and thus reduce the number of mowings
► To stimulate root formations, as for the rooting of cuttings
► To accelerate ripening, and thus permit growers of heavy produce like pineapples to stagger ripening to fit canning schedules

For many of these possibilities major credit goes to the Boyce Thompson Institute's Dr. Zimmerman. The economic potential is beyond present calculation; yet there is scarcely one of these applications that would not have been thought a crackpot idea a decade or two ago.

The fungicides

Fungicides do most of their work in orchards, vegetable fields, seedbeds, and greenhouses, and on the seeds themselves. They have entered an area that used to be extremely confusing to the farmer. Was tobacco "mosaic" a disease or an act of God? What was the cause of "damping off" in seedlings? The study of plant pathology has taken big strides during the last decade, and the agrichemical industry is striding with it. Expert opinion, hopelessly divided on the question of what are the six most destructive insects or weeds in U.S. agriculture, has no trouble at all agreeing on the three most important diseases caused by fungi: they are apple scab, late blight of potatoes (the cause of the Irish potato famine in 1846), and the stem rust of wheat. The control of the first two has now entered the same revolutionary phase as the control of weeds and insects; control of wheat stem rust is, says Dr. Richard H. Wellman

of Union Carbide and the Boyce Thompson Institute, "one of the brightest will-o'-the-wisps currently leading research workers in the field."

Chemically, most of the new fungicides are carbamates: complex entities that, like the insecticides, combine rings and chains of atoms in the same molecule. Tucked away in all of them are occasional inorganic atoms of sulfur, chlorine, or iron. Phenyl-mercury compounds—such as PMA—are not good protectants against fungi but find high value wiping out infections after they have occurred. All these new products are cutting heavily into the old-fashioned copper salts and lime-sulfur mixtures of the past. In 1951, out of 600 million pounds of all fungicides sold, only 13 million pounds were organics—but once again, since the new compounds do so much on so little, the figures are deceiving. The chemical firm of Rohm & Haas pioneered in bringing the new fungicides to their present market volume.

Seed treatment

Not only fungi but bacteria are now being isolated as the causes of many hitherto obscure plant misfortunes, and the same families of chemicals that act as fungicides are also intensely useful as seed disinfectants and protectants. Soil microorganisms that attack seeds are responsible for hundreds of different plant diseases, and the disinfection of seeds has now become highly important business.

As an example of how one step in agriculture leads to another, it was the development of hybrid corn, in which that unhappy political figure, Henry Agard Wallace, took such a brilliant part, that stepped up the demand on the chemical industry to develop seed-protection chemicals. Hybrid-corn seeds are extremely expensive compared to the rugged old horse corns of the past, and their disinfection became essential as insurance to the farmer that his seeds would not succumb to fungus attack before their cotyledons appeared. Recently, a Kansas official estimated that in his state

"modern methods of seed treatment have *saved* 5,000,000 bushels of wheat, 7,500,000 bushels of oats, 5,800,000 bushels of grain sorghum, and 370,000 bushels of barley: to a total value of $28 million." One ounce of disinfectant, costing a few pennies, may protect enough seed to sow a dozen acres. "There is no agricultural practice," says the du Pont company, measuring its words as usual, "that yields as great a return for so small an investment of time, labor, and money as seed disinfection."

Soil conditioners

Led by Monsanto Chemical's Krilium, soil conditioners first went on the market in 1952, and some thirty products, legitimate and illegitimate, are before the public now. Soil conditioners are *not* fertilizers; their function is to make the soil more friable. By so doing they can make a soil more stable under the effects of rainfall, prevent surface baking and crusting, increase the absorption and retention of water, improve drainage and aeration, and lessen the effects of erosion. Their enthusiasts think they might increase the yield of root crops by between 50 and 100 per cent.

Conditioners have caused a great deal of public excitement, but so far as U.S.D.A.'s Beltsville Station is concerned, they are still on probation. But before these new plastics—for that is essentially what they are—can show what they can do, their prices will have to come down. The cost of conditioning 100 square feet of soil is now around $2.50, which is very high. Meanwhile, however, research into conditioning chemicals goes on and on: thousands of chemicals are being screened each year for their possible soil effects, for the possibilities are enormous. There are hundreds of thousands of acres of "tight-clay" soils up and down mid-America, which are, in the farmer's phrase, "too wet to work before dinner and too dry to work after dinner." So far, legumes, chemicals in combination with legumes, and chemicals applied as conditioners have not improved the soil-water-humus relationship enough to make *these*

lands profitable in the long term, particularly for row crops; too big an investment in power and equipment is needed to prepare and plant them. But if soil conditioning can improve tilth and productive capacity, the dollar costs involved *might* be reduced as much as one-third on a vast number of farms, particularly on alluvial land areas like the Mississippi Delta.

And so?

A roiling turbulence of countercurrents is today flowing through American agricultural-industrial life. The U.S. is a great country, but two-fifths of it, agriculturally speaking, is arid—and of the almost 2 billion acres that make up the continental U.S., only 400 million are now cultivatable. Since 1940, 1,500,000 farm workers have left the land for offices and factories—and so far in this century farm labor has dropped from 38 per cent of the total labor force to 12 per cent. But this fixed patrimony and a declining number of workers have, thus far, been a problem splendidly met.

A hundred years ago one farm worker could produce food and fiber for fewer than five people; by 1910 he could produce enough for eight; by 1950 he had reached the point of being able to feed, and partially clothe, almost fifteen. "The increased output for human consumption during the last thirty years," writes du Pont's Dr. Goebel, "has been achieved one-third by mechanization and two-thirds by increased productivity brought about basically by application of biological and biochemical research to agriculture."

But even this great achievement must now be surpassed. The President's Materials Policy Commission assumed, in writing its 1952 report, that by 1975 the population of the U.S. would be 193 million. Its consultants showed good reason why it would be possible for the U.S. farmer to increase food and nonfood yields by better than 100 per cent by 1975, from the fixed acreage we have today. And this is what the farmer is going to have to do, if an increasing domestic population is to stay on a rising curve of living

standards, and the U.S. is to do what is practical and prudent to share its plenty with that portion of the free world which it does not propose to abandon to Communism. In today's mighty effort to increase food supplies, and the techniques to enhance them, mechanization is straining hard, but the chemicals have only now begun to fight. It is on their shoulders that the great future burden will rest. Talk of temporary crop surpluses, undoubtedly forthcoming, seems cheap indeed in the face of the future world's need and hunger.

November, 1953

10

Power from the Sun

BY ERIC HODGINS

America is using up its heat-producing raw materials. Conservationists make it plain that we must tighten the limits on our use for today and, for tomorrow, look for ways to tap new sources. Two rich veins of heat-energy remain to be mined for general use: one atomic, the other solar. Of the two, sunlight holds the greater ultimate promise, but it is proving the more difficult to harness.

"Suppose all the earth's coal, lignite, peat, tar sands, crude petroleum, natural gas, and oil shale that we are ever likely to produce in the future . . . were collected and that all our timber were cut into cordwood. Suppose we segregate all the uranium and thorium that we are likely to produce in the future . . . and that it is all purified for nuclear fission. Thus we have at hand for immediate use all the earth's stock of fuels. Then, suddenly, we extinguish the sun. We ignite our fuel in such fashion as to give us energy at the rate at which we are accustomed to receive it from the sun. In about

three days our entire supply of combustible fuel would be gone. Then we would get the nuclear reactions under way. This would last us less than an hour if the 'breeder principle' could be applied—otherwise only a few seconds. At the end of a few days the earth with its load of ashes and radioactive wastes would begin its descent toward some temperature only slightly above absolute zero."

Energy Sources—The Wealth of the World
Ayres and Scarlott (McGraw Hill, 1952)

The sun whose rays are all ablaze with ever living glory is not likely to go out—but if it did the consequences would be immediate, and offer no time for the prudent preparations suggested above; the radiations that burst from the sun at temperatures up to 500,000°C. take only eight minutes to reach the earth. The cause for worry over the future lies less on the sun than on the earth, where rising population pressures and a desire—particularly in the U.S.—for consumption on a scale that would eventually grind up the Rock of Ages for toothpaste are straining our capacities to supply the necessary materials, food, and energy to keep ourselves going. In the hundred years that ended in 1950, Industrial Man consumed two-thirds as much energy as was used throughout the entire Christian era of the preceding eighteen and one-half centuries. That is a world figure: in the U.S. we now consume fifty times more energy per year than we did when Thomas Jefferson was President.

As we came to depend more and more upon the burning of coal, oil, and gas, we began to permit a state of affairs in the world of energy that would horrify even the most complaisant in the world of finance: today, except for that tiny fraction of our total energy that comes from fireplaces and dams, we live on a capital-dissipation basis. We can keep this up perhaps for another twenty years before we begin to find ourselves in deepening trouble. Since the sun is responsible for the existence of everything living on earth, and the actions or conditions of everything else, it is to this star, so

minor in the universe but so important to us, that thoughtful scientists now increasingly turn, for help in a jam.

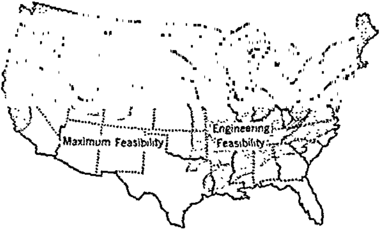
The sun offers to the earth a bounty, an inconceivable bounty, of a million trillion kilowatt-hours in the course of a year. Sixty per cent of this the earth immediately rejects; the radiations strike its heavy cloud envelope and are absorbed or returned to space. The balance, a mere 400,000 trillion kwh, strikes land and sea, where again the largest fraction bounces off. But some 120,000 trillion kwh becomes the means to the greatest mass-production phenomenon known to man, which man has contemplated, first stolidly, then with wonder, and today with respectful but utter helplessness. This mass production can be described, in terms of a freshman chemistry textbook, as:

$$CO_2 + H_2O + \text{light} \longrightarrow C_n(H_2O)_n + O_2$$

This means that, under the influence of sunlight, plants combine carbon dioxide and water, with chlorophyll acting as a catalyst, into carbohydrates of many forms, all of which have the advantage to man that they can be burned, either in living stomachs or in other converters, for the production of energy. The burning releases water and carbon dioxide back to the atmosphere for more "photosynthesis" by the vegetable kingdom, which knows so much more than man, but keeps so silent about it. Each year, as Eugene Rabinowitch, research professor of botany at the University of Illinois, points out, "the plants of the earth combine about 150 billion tons of carbon with 25 billion tons of hydrogen, and set free 400 billion tons of oxygen." He goes on to say that "few are aware . . . that over one-half—perhaps as much as 90 per cent—of this giant chemical industry is carried on under the surface of the ocean, by free-swimming microscopic algae"—and thus far beyond human ken.

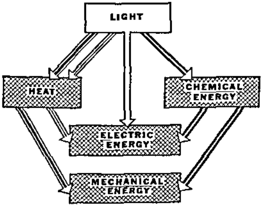
Yet it is by courtesy of this giant chemical industry that man lives: it supplies him with his planked shad and roe, his broccoli, his T-bone steaks; the gas and oil and coal by which he cooks,

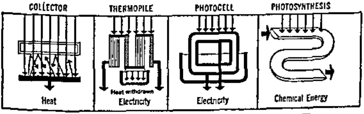
Solar Energy



Slowly the U.S. is waking up to the fact that sunlight could be made to heat houses and provide domestic hot water if as much engineering thought went into the design of solar heaters as now goes into air conditioning. The map shows that in two-thirds of the U.S. the task is easy, or almost easy.

Light is life, but only the vegetable kingdom knows how to obey this law directly. Man, lacking the cauliflower's instinct, must travel by serpentine paths: (A) convert *light to heat* in flat-plate collectors and use the heat to produce mechanical energy via heated water or air; (B) convert *heat to electricity* directly via a thermopile of dissimilar metals; (C) convert *light to electricity* via the photoelectric effect, such as that which activates the photographer's light meter; (D) find closer means of controlling nature's own photosynthesis reaction, of *light to chemical storage* of energy, in some convenient form that man can burn or eat, and re-create at will.





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endures the weather, and transports himself. But how much of this giant industry can he at present make any use of? One clear calculation comes from Farrington Daniels, professor of chemistry at the University of Wisconsin. Professor Daniels' calculations, published in 1950, show that in the U.S. the year-round Maine-to-Arizona average of energy from the sun amounts to about 1 kilocalorie per minute falling on 1 square foot of land. (A kilocalorie is the heat evolved by burning half a wooden match.) In larger terms this means that 280 million kilocalories fall, as a daily average, on 13 acres of "average" land in the U.S.—the 13 acres being a significant figure because that is the area to which each dweller in the U.S. is statistically entitled (U.S. continental land area of 2 billion acres divided by a population—at the time of Daniels' calculations—of 150 million). This is, so to speak, the American energy birthright—varying markedly from place to place and from time to time, but subject to systematic depression only by the rise of population.

It is not a birthright of which we take much advantage. In the well-fed U.S., one person's daily food intake is perhaps 3,000 kilocalories.* The coal and oil and gas burned in the U.S. annually, again divided by the population (and by the days in a year), give a figure of 147,000 kilocalories to keep things going for one individual. A daily individual total of 150,000 kilocalories, then, sustains our present standard of living. But Professor Daniels has shown that we have available to us not this paltry figure, but something nearly 2,000 times as great: those 280 *million*. The tantalizing fact, then, is that despite Henry Ford, Macy's, General Foods, Consolidated Edison, Herbert Hoover, Grand Coulee Dam, and your friendly Mobilgas dealer—to name only a few instrumentalities of human progress to date—we so far hobble along on only 0.05 per cent of the energy offered us by the sun, and let all the rest slip from our grasp.

* *When physicians and dietitians speak of a "calorie" they mean a "kilocalorie," thus introducing a thousandfold confusion into the world of chemists and physicists.*

A valiant struggle is now going on to see if it is not possible to improve this operating factor. At present the struggle takes two principal forms: engineering research into the design of cheap heat collectors to provide energy for running engines, evaporators, stills, etc.; and scientific research into the deeper mysteries of photosynthesis. "There are many different ways," writes Eugene Ayres, the brilliant and learned author of *Energy Sources*, "of capturing [solar] energy—from desert areas, peat bogs, fertile valleys, forests, oceans. If photosynthesis can be manipulated to take care of the world's food requirements, similar industrialized techniques may be applied to the growth of fuel. Superior photochemical reactions will probably be found. We are only at the threshold of knowledge of such phenomena."

The trouble with the sun

We are indeed. The better use of sunlight has been bedeviled for some time by a couple of misconceptions. For one thing, sunlight is "free" just so long as we do not try to catch it; the catching turns out to be an expensive business. For another, the future world that *does* capture solar energy is not likely to be a straight-line projection of our present highly urbanized U.S. factory-system economy for the driving of high-powered machines by means of high-temperature engines and high-speed generators of high-tension alternating current. Rather, the increasing use of solar energy would seem to be for the benefit of home and farm, and for spreading more usable energy into the hitherto underpowered areas of the world via machines operating at lower temperatures and slower speeds, and perhaps producing lower voltages of old-fashioned direct current. The feelings of a city dweller in mid-August notwithstanding, the trouble with the sun as an energy source is that the temperatures it produces on the earth are not very hot—at least by the standards of a conventional heat engineer.

It is significant that, among nations, neither the fast-spinning U.S. nor sun-stunned India takes the highest advantage of solar

radiation: this distinction belongs to a tight-pressed nation with an old but active culture, living well in spite of a population density higher than that of any other major European state (0.85 acres per person against the U.S.'s thirteen), and two and one-half times as high as India's. This is Holland. The high level of agricultural productivity in Holland, despite heavy geographical drawbacks, attests the Dutch use of the sun as more efficient than any other nation's.

They do it, of course, by intensive and intelligent farming, which includes high use of irrigation on reclaimed land and a good deal of low-potential local power. (With so much of their land below sea level, the Dutch hydro-power potential is eternally nil.) The Dutch proficiency in agriculture demonstrates that it is not necessary to understand photosynthesis in order to profit by it. In whatever country, the farm and the forest are the only present-day converters of solar energy of major importance in daily life. The President's Materials Policy Commission in 1952 published estimates from various agricultural economists indicating that crop yields in the U.S. could be increased anywhere from 70 per cent to 200 per cent, *without any change in present acreage*, if all farmers could be persuaded to use the maximum economic amount of fertilizer and otherwise lift their operations to the level of the best practical procedures currently known. But this increased utilization of solar radiation would be achieved without in any way reaching the heart of the scientific problem.

The best scientific thought in America seems at present to hold to two major propositions. The first is that in the long, the ultra-long, run, solar energy will beat out atomic energy as the prime mover of the future world, although both are at present just about equally uneconomic compared to the sources to which we have been accustomed since the Industrial Revolution. The second is that, as between direct collection of solar energy and its better utilization by means of greater understanding of photosynthesis, direct collection ("the bullheaded way" in the language of Vannevar Bush, a

pioneer in the field) will have the earlier triumphs, but will be superseded eventually by carefully controlled photochemical reactions either within or without the living cell.

But it will take large drafts of money and of brains to bring these prophecies true. At present, the domain of solar energy is the most neglected in all science. "Our children's children," wrote Eugene Ayres, "may be puzzled when they read that in 1951 nearly two billion dollars was spent for scientific research under U.S. Government auspices, but less than one-hundredth of 1 per cent of this sum was devoted to solar-energy problems." Mr. Ayres is here referring to the $150,000 to $200,000 being spent largely by the Atomic Energy Commission, with a dab of help from the National Science Foundation and the Office of Naval Research, which is the financial limit of interest at present expressed by government.

Among private sponsors of solar-energy research only two individuals stand out. New England's wealthy Godfrey Lowell Cabot in the late 1930's gave a little over $600,000 each to Harvard and to M.I.T., the income from which was to be used for solar-energy studies. Since the war, Charles Francis Kettering, retired General Motors vice president and director of research, has spent about $5 million of his own funds, mostly on photosynthesis, establishing research centers at Antioch College in Yellow Springs, Ohio, and at his home in Dayton, and supporting research in a dozen other institutions. Solar-energy research is not a field in which business corporations have shown any interest: unless the chemical industry persuades itself that here is a field in which large research appropriations may later pay off in dividends, solar research will continue to depend on wealthy individuals, although it is hard to think of any research field more invested with a public interest. The fact that the research is bound to extend into chemistry, physics, biology, botany, and numerous subspecialties demands a powerful research administration to make things happen across the board. There is no hint of this in the Washington of today.

The big energy need

Humanity's primary need for energy is, pathetically enough, just to keep warm. Even in the high-fuel-consuming, power-hungry, super-mechanized U.S., the largest single consumption of fuels of all kinds goes to what is called "space heating," which uses over three times as much fuel as the operation of all the country's railroads; over twice as much as the running of all its automobiles, trucks, and airplanes; and 15 per cent more than the total of its manufacturing and mining operations. So it would certainly seem to be here that a better utilization of solar energy should begin—all the more so because space heating does not call for high temperatures. Yet it is beginning extremely slowly, and the kind of product engineering and evangelical salesmanship that has been poured into refrigerators, home freezer units, and air conditioning is wholly lacking on the other side of the heat barrier.

The use of large, south-facing windows—preferably with two thicknesses of plate glass, factory sealed—and a more general understanding of the principles of house orientation and ventilation are two simple essentials to a more practical, immediate use of the sun's bounty. A third, which has yet to appear in wholly satisfactory form, is a system for the long-term storage of heat energy so that it can be tapped, again as heat, when it is wanted. In the chilly latitude of Boston, Dr. Maria Telkes, formerly of M.I.T. and now of New York University, has shown what can be done in the cause of solar space heating through the judicious combination of publicity and Glauber's salt in the experimental Telkes-Peabody-Raymond house in Dover, Massachusetts. Glauber's salt, a dehydrated form of sodium sulfate, melts at a temperature of 90°F.; in so doing it absorbs generous quantities of heat supplied it by solar collectors on the vertical south wall of the house; when it solidifies it gives this precise same quantity of heat back to its surroundings. It is not a perfect system, but 20 tons of Glauber's salt, in cans packed into the "heat

bins" of the Dover house, hold its temperature relatively level and make the house livable without standby heat through 95 per cent of a New England winter. With some further work, salt hydrates might eventually supply hot water for bathing and doing the dishes. Tanks of water and large beds of pebbles or stones can also be used to store heat, but, says Dr. Telkes, "with salt hydrates sufficient heat for an 'average house' in the New York–Boston–Chicago zone may be stored in a small cubicle; to provide comparable heat storage capacities, water may require an entire room, and stones half a house."

Although the storage system is still the weak link in solar space heating, the main impediment to it today is a strange socio-industrial lethargy. Fully one-third of the U.S. falls into a zone of maximum feasibility—that is, all heating requirements can be met by solar radiation—and another third is a zone of "engineering feasibility," meaning that solar heating is a little more expensive but not too difficult. Only the dwellers in northern New England and, in general, the Canadian-border states need to continue to rely, as the rest of the country supinely does, on the combustion of fossil fuels for keeping dwellings warm.

Why a state like Florida, where population is growing at a tremendous rate, and where the incidence of sunshine is almost as remarkable as the state chamber of commerce would ask you to believe, has thus far attracted so little attention from any intelligent sector of the plumbing, heating, and air-conditioning supply industries is a matter that economists and sociologists must explain, for the barriers do not lie in engineering. The statement is heard again and again in solar-energy discussions that there is "wide use" of solar-heating equipment in Florida, but except for some few installations on the state's east coast, dotted from Palm Beach to Miami, this is simply not so. No sillier spectacle of a perfect laboratory for practical solar experimentation going almost wholly unused can be found anywhere, except perhaps in India. Moreover,

the sun can just as well be used to power an air-conditioning and cooling cycle as to supply heat directly—and as has been pointed out by George O. G. Löf, a chemical engineer wise in the ways of the sun, it is when the sun shines hottest that people yearn most to be cool. The first cost of solar heating or cooling equipment is high, but this is so because insufficient industrial thought has been given to the efficient design of high-volume-produced equipment to take a wholly unnecessary burden off energy sources that must be lugged bodily into the state and set fire to, while the sun's energy spills all about and runs off the earth's edges to warm the tropic moon.

Suppose now that solar energy is to be used, by direct collection, not for heating but for power. Again, the sun's low temperature as perceived on the earth is a problem. Its heat is abundant but diffuse, and the desire to produce high concentrations of heat, important for thermodynamic efficiencies, runs head on into a still more important consideration—that the collector be as cheap as possible in both investment and operating cost. The past history of solar engines has brought forth great designs which have contemplated parabolic mirrors to focus the sun's rays in a small spot of extreme high temperature and have even contemplated turning these mirrors on stems by clockwork—or, later, electronic means—to keep their axes pointed straight at the sun as it traverses the skies. Such pieces of massive technological jewelry can justify their expense for special purposes, such as smelting or metallurgical operations where heat involving incandescent carbon (as coal, coke, or the carbon electrodes of an electric furnace) cannot be used. Such an installation is Félix Trombe's solar furnace in the French Pyrenees, which can easily produce from the sun a concentration of "pure" heat of 5,000°F. or better, but it has no bearing on the future of widely used power from the sun.

The same criticism applies to the great pioneering work done in the U.S. by Dr. Charles G. Abbot. Dr. Abbot, now secretary emeritus of the Smithsonian Institution, has devoted a great part of

his lifetime to solar-energy research and has produced a solar engine that consists of a parabolic collector focusing the sun's rays on a flash-type steam boiler. Dr. Abbot's engine, now in the Smithsonian's archives, is estimated to cost $1,000 in 2 horsepower size and could under favorable circumstances supply power for about 10 mills per kilowatt-hour: since the average generating cost for electric current in the U.S. is only 7 mills, this is too high except for special small-output uses in power-short areas. Engineering thought is now well agreed that the future of economic solar collection does not lie with mirrors and focusing devices. Instead, it lies with a simple black box.

Greenhouse principle

This box can be of any dimensions you please, except that it should not be more than a few inches deep. Its bottom is made of blackened metal, and its top is fitted with a lid of glass. It is, in effect, a miniature of an antique invention: the greenhouse. Inside such a box the temperature will readily rise to 300°F. or better, and through a series of such boxes, perhaps occupying not merely square feet but acres, water or air can be circulated to carry this moderate-temperature heat off for whatever the purpose at hand.

If it is this simple, why is not the problem of the direct collection of solar energy solved? It is not solved because, even after fifty years of experiment, nobody yet knows how to make the black box cheap enough. Two sheets of glass, sealed together with dead air space between them, make a much better lid than one sheet—and four are better than two. But glass is expensive—particularly the iron-free glass that serves best. Again, the box will collect much more heat if the glass is coated with a fluoride or silicate, as camera lenses are coated, or has its surface etched to reduce total reflection (that is, reflections that are squirted away by the glass and never get inside the box at all). Treated glass, such as has been used experimentally in the solar house-heating project at M.I.T., could

be turned out in large quantities for about 20 cents a square foot. This is 5 cents higher than the cost of good iron-free plate glass, which runs into money fast.

At M.I.T., Professor Hoyt C. Hottel, chairman of the Cabot committee on solar-energy utilization, has tentatively concluded that, using four thicknesses of treated glass, 580 square feet of collector would be needed to produce a steady kilowatt of electrical capacity, in the extremely favorable latitude and climate of El Paso, Texas. Anyone with ambitions to construct the equivalent of a 100,000-kilowatt generating station "fueled" by solar collectors would thus face a bill of $46 million for glass alone, to say nothing of the cost of using the 1,330 acres of land necessary for collectors.

The high cost of sunlight

If it were possible to construct a collector, plus the necessary piping, for $1 per square foot—which would yield 15 kwh per year—then the "fuel" cost of each kwh would come to between 6 and 7 mills. "This is, at present, a magician's cost for collectors," says Professor Hottel, "but even assuming it were real, it should be sufficient to prevent the public from getting the idea that sunlight is free energy."

It so happens, in the U.S., that where sunlight is abundant, fossil fuels are at present cheapest; in the Southwest, for example, the *fuel* cost of power is around 2.5 mills. Does this comparison mean that power from direct solar collectors is an impossibility? Not at all. It does mean that at present solar energy for power is relegated to those same areas where nuclear energy is at present also economic: those parts of the world—or the U.S.—where fuel costs are considerably higher than they are throughout most of this country. In countries like India, or Africa, or parts of Latin America, where great treasure-troves of materials await development in the interests of the free world, the possibilities are much more immediately hopeful.

Power for the desert

Neither here nor abroad is an equation between directly collected solar energy and high-tension alternating electric power the most likely coupling to look for. A most logical use for direct collection, next to space heating, would be the pumping of water for irrigation—for here the troublesome storage problem is eliminated, and the intermittency of sunlight is not a problem. "Any scientist who visits one of the deserts of this earth," writes Arthur von Hippel, of M.I.T., "will be struck by the paradox of the situation: glaring sunlight burns the last traces of vegetation off the ground; but below the sand, there is water in many places that could be pumped by the same solar energy to the surface and transform the desert into a Garden of Eden."

Dr. Vannevar Bush is now proposing that in many places irrigation can in the future be accomplished much more simply and inexpensively by the use of a slow-moving heat engine—in effect a combined air compressor and pump. It is not only slow but, from the standpoint of engineering design, deliberately and happily crude. Essentially this heat machine consists of two tanks, roughly 5 feet in diameter and 15 feet high, connected at their bottoms by a U tube. In these tanks, water, about 10 tons of it, has been set to surging. Heated air from black-box collectors is sucked through a valve into tank A by the downward surge of water. After the inertia of the water in tank A has carried its level below that of tank B, another valve opens and sprays cooling water into tank A's air space. The air contracts, and the surge of the water reverses. Air in tank A is thus now compressed and some of it is forced through an outlet into a storage tank. The net output of this "surge-pump" type of machine operating on hot air at atmospheric pressure is a supply of cooled *compressed* air, which is then used in an air-lift pump to raise water for irrigation.

The success of the system will depend on the cost of the flat-plate

collectors. But if the economic details work out as now seems possible, a considerable short cut in irrigation methods will be in the making. The Bush heat engine would not only operate slowly, but intermittently, upon the whim of the sun. In so doing it would avoid some of the costs we must pay today because so much of our power is always ready for use whether we want it or not.

Other paths

Everything discussed so far relies upon the direct collection of solar radiation and its conversion into mechanical and thence, if desired, into electrical energy. But there are other possible paths that involve neither "the bullheaded way" nor the penetration of the mysteries of photosynthesis within the living cell. The trouble with most of them is that they are not as good as they sound—at least not yet. There is, for example, the thermopile, which converts heat to electricity directly when one set of junctions of dissimilar metal wires is kept cool and heat strikes the other set. The thermopile is in wide industrial use for the measurement of temperatures, but it shows no present disposition to provide quantities of electricity larger than those that will move a delicate galvanometer needle. There is also the photoelectric cell, which converts light to electricity directly (as in a photographer's light meter), and this would certainly become the world's greatest convenience if it could be made to produce currents in large quantities and efficiently. Perhaps someday it will. Perhaps someday the whole idea of *the battery* will improve enormously over today's concept of small power-storage units that crank an automobile or light a flashlight bulb from energy previously stored in them. The so-called "wet photovoltaic" battery at present offers a possible form. It is essentially a battery in a transparent case. The electrolyte around one electrode is exposed to the light; when the other is kept dark, an electric current flows. The day may come when batteries will be able to supply great quantities of electrical energy, as direct current,

at high voltages, via an electrolyte that is a free-flowing stream between the battery, where the energy is released, and the regeneration side of the cycle, where light energy is converted to a chemical form for storage.

The lack of ability to store energy successfully is a sore lack. In one sense, of course, a chemical company stores energy when it converts raw materials, via heat, into a bag of fertilizer—but such forms of storage are not freely circulating convertible currency in the energy world. In the world of theoretical physical chemistry there are many reactions by which a *temporary* energy storage can be achieved; the trouble with almost all of them is that the reactions would as lief run backward as forward and no means has been found of making them reversible only at man's convenience. For this reason it could be that an experiment begun at M.I.T. back in 1945 may have highly important consequences.

Light to chemical storage

This was an experiment made in the department of physical chemistry under the direction of Professor Lawrence J. Heidt. In effect, Professor Heidt and his associates decomposed water into its gaseous constituents of hydrogen and oxygen by means of a beam of light—in this case monochromatic ultraviolet—thus for the first time successfully using light to produce chemicals that will return, on demand, the energy they have stored. This decomposition was achieved in the presence of a couple of complex inorganic chemicals, perchloric acid and ionized cerium salts, which, although essential to the reaction, were unchanged by it, and thus performed the functions of a true catalyst—by analogy playing the same part in this "nonbiological photosynthesis" that chlorophyll plays in breaking up carbon dioxide.

The cerous ions in the solution do not react to the full spectrum of sunlight but only to that 4 per cent which comprises the ultraviolet radiations—and the usefulness of Professor Heidt's experi-

ment has been questioned on this ground. It has not yet progressed beyond laboratory experiment, so no one has any idea of the cost or elaborateness of the equipment needed to reproduce it on any sort of going basis. But its commercial implications are clear: hydrogen and oxygen, weight for weight, provide the most violently energy-releasing chemical mixture known to man when they recombine into water: and the temperature of the hydrogen-oxygen flame in controlled combustion is extremely high. The hydrogen-oxygen flame could be used to heat a fluid for a conventional heat engine, but a more attractive idea might be to develop an extremely high-temperature internal-combustion engine that could be a distant cousin to the jet engines of the present day.

It is too soon to begin designing a heat cycle or a new kind of "explosion motor" to take advantage of Professor Heidt's discovery; its full possibilities are likely to be known only when some large industrial enterprise endows it with sufficient money to test its practicality. This idea, of course, involves "photosynthesis" just as much as does the growing of a piece of sugar cane or a eucalyptus tree, except that it exists entirely apart from living cells.

Back to nature

If the free world is to have a rational development during the rest of this century, power will be needed mostly where there now is little or none. Dr. Bush's heat engine for irrigation is one example of how solar power might be brought to aridity. Another example lies in solar stills to produce potable fresh water from the sea. This was a wartime project for saving lives of aviators or sailors adrift on rafts, but it has great applicability to the peacetime development of tropic arid lands near the sea.

The idea of adapting engines and other energy converters to the fuel on hand in a given area is growing. It is a recognition of the world's steadily increasing need for power on terms that represent

a shorter connection between solar energy and the energy man can use than is represented by coal and oil deposits that have been geological aeons in the making. In England now, for example, there is being developed the so-called "Ricardo engine." The most startling thing about the Ricardo engine is that there is nothing startling about it at all: for it is a small, slow engine, capable of producing not much over one horsepower, and is designed to burn wood for fuel. But the wood is eucalyptus wood, and the Ricardo engine is designed principally for use in India.

In India the tree *Eucalyptus globulus* grows at a rate of almost 10 tons of combustible wood per acre per year and has a heat value a little more than half that of the same weight of bituminous coal. The Ricardo engine will have an enlarged boiler and a draft arrangement to keep the easily extinguished fuel alight. Dr. Edward C. Bullard, director of Britain's National Physical Laboratory, has calculated that, at least in theory, about 8 square miles of eucalyptus forest would perpetually provide enough fuel for a wood-consuming power station to supply 10,000 continuous kilowatts of power. The Ricardo engine, designed for lighter tasks than this, such as pumping water and driving light machinery, would have a much lower efficiency, but would provide some improvement over the bullock, which is the standard of rural power in India, and which must be continuously fueled whether it works or not.

This new emphasis on the old, exemplified by the Ricardo engine, underlines an important point in all solar-energy-conversion studies: the costs of assigning an acre of land to agricultural growth and then burning the resultant crop remain lower than the costs of installing the cheapest presently imaginable direct collectors on the same acre, and using the entrapped heat. However, getting power from eucalyptus trees, for example, demands a land area devoted to their growth forty times greater than the area needed to acquire the same amount of power from the sun via flat-plate collectors—if

only the cost of the collectors would permit it. Here, to an engineer, is at once the enticement and the frustration of solar-energy calculations today.

Photosynthesis

So far, nature knows best. But nature won't tell. Until recently, the chemists, biologists, botanists, and physicists delving into the intricacies of photosynthesis were badly stuck: they could not duplicate any of the reactions in the living plant by test-tube means; nor could they even agree on the order and complexity with which these reactions occurred in nature. Assume, said Professor Rabinowitch, in recounting past efforts, "that chemical methods of fractionating the plant cell contents are too drastic, and [that we should] attempt instead to take the cell apart by mechanical means. We take a giant green cell, such as are formed by some algae, and prick it with a needle in an attempt to reach its interior. Immediately photosynthesis ceases. Thus we find ourselves in the position of being asked to find out how an automobile operates without being permitted to lift the hood."

This situation is now slowly improving. By means of the radioactive isotope Carbon 14 it is now possible for biologists to know much more about how atoms of carbon arrange and rearrange themselves in the enormously complex molecules that make up living substances. Research workers are now convinced that a plant performs two functions: one as a power storehouse, the other as a chemical factory. But in this whole domain there is at present raging a heavy scientific controversy. There is, for example, no agreement among scientists as to the maximum efficiency of the natural photosynthesis process. Otto Warburg, a German biochemist of high distinction, affirms that this efficiency is 75 per cent or even better; various Americans are certain that the correct values are closer to 25 or 30 per cent. There the layman must let the matter rest, aware that present-day agriculture has a long way to go to catch up with

the ideal; a well-harvested corn crop, for example, yields back to the farmer perhaps 1 per cent of the solar energy that was available to it.

Chlorella

While one group of photosynthesists sweat and quarrel over their enormous tasks, another group are interested in how nature could be manipulated for higher food or fuel yields to meet emergencies that might occur before a truer understanding dawns. Man derives a sizable portion of his nutrition from algae and algal substances generally—but usually indirectly, as where he feeds on fish nourished by algae. A single-celled green alga, by name Chlorella, has long interested biologists, principally because it can be propagated with remarkable speed and ease. In 1950 the beginning of the Korean conflict gave Chlorella a sudden importance in the geopolitical-energy world. While the U.S. was preparing for almost any eventuality, a group of scientists, many of whom had been a part of the Manhattan Engineer District in World War II, cast about to find a large-scale practical objective for intensifying the work they had since been doing on photosynthesis. Japan's usefulness as a base for United Nations forces was heavily threatened by possible submarine warfare, since a quarter of all its food must be imported; therefore a massive research program designed to make the Japanese food supply less vulnerable to blockade presented itself as something of high importance. Many scientists were attracted to the idea because it seemed to provide militarily useful research that would have an even greater value after the conflict; "it was atomic energy but it wouldn't blow up." Although World War III did *not* develop from Korea—not then, at any rate—the photosynthesists' proposal stimulated Japanese scientists to turn their attention to algae culture, and specifically to Chlorella. For Japan this seemed a thoroughly practical development; the population is used to eating seaweed (marine algae) in its party dishes, and the proteins in Chlorella have a constitution very similar to those in the soybean.

Algae in Japan

At the same time, interest in the idea of cultured algae entering into the food-energy equation of the world arose in the Carnegie Institution of Washington, and resulted in a good deal of practical work on its propagation; the firm of Arthur D. Little, Inc., in Cambridge recently reported back to Carnegie on a set of experiments conducted on its behalf. The intensive growth of Chlorella is essentially an exercise in hydroponics, i.e., the cultivation of living materials without soil but in nutrient streams of water. A Chlorella farm of the future might envisage streams of nutrient solution, with the growing algae suspended in them, flowing through flexible, transparent tubes of a plastic like polyethylene, laid out acre after acre, absorbing sunlight and reducing carbon dioxide to energy-bearing substance.

By varying the nutrients it is possible to breed Chlorella selectively for high-fat or high-protein content. The Little experiments produced a yield of something like 15 tons of dry-weight substance per acre per year, and pointed to 35 tons as a possibility. In Japan a shorter-term experiment produced a yield equivalent to 30 tons per acre per year. "The method used in Japan," said Professor Hans Gaffron of the University of Chicago, writing in the British magazine *Research*, "was to let the suspension [of algae and nutrients] flow and circulate through open concrete ditches, periodically recharging it with carbon dioxide in a tower where air enriched with carbon dioxide was bubbled through the liquid." If the Little experiments give a guide to the future, algae can be produced for about 25 cents per pound of dry weight. This is still too high to make sense, but at 15 cents a pound algae could become part of the world's food-fuel supply.

This is certainly a new world, if not particularly a brave one. It is one in which Professor Gaffron makes the inference that Europe is slowly on the way to becoming vegetarian, to say nothing of those countries like Japan and other parts of Asia, or Puerto Rico: "It

is, of course, impossible to predict *how soon* the countries of Western Europe will become either so poor or so overcrowded that dairy products will no longer be available . . ." But apparently this is inevitable, at least if man does not show more determination about the nature of his future than he seems disposed to show so far. "The reporter of the future," says Professor Gaffron, "will certainly hail it as a great achievement when our crowded great-grandchildren shall subsist contentedly—because they know no better—on hydrolysed sawdust and predigested, vitaminized algae. But we, should we not rather strive to preserve for them conditions where they may still be able to find a garden in which to pick fruit from a live tree?"

There is more than one way of saving ourselves from a future in which the world is long on population and short on everything else, most notably foods and fuels. It has been suggested, and by many of the authorities quoted in this article, that massive governmental funds, on just such a scale as went into the production of the atomic bomb, be applied to every aspect of research to improve solar-energy utilization. But there is also a countersuggestion. It is made by M.I.T.'s Professor Hottel, questions: "Should we suggest that the government spend money in a difficult search for more power rather than an easy search for better use of presently available power?"

The latter would at least postpone a day of reckoning if it is within the American temperament to carry it out. Of total U.S. energy consumption, we use up 50 per cent just for space heating and automotive transportation. We have, today, says Professor Hottel, "enough technical know-how to cut in two our energy consumption in these two fields, and might well clinch the application with a modest amount of research. To carry, on the average, perhaps 1.2 passengers in a 3,500-pound vehicle hardly makes sense; and to refrain from building houses so that they require half their present fuel for the same usable floor occupancy also seems unreasonable."

It does—but whose will be the first hand to reach to close a valve?

September, 1953

11
The Peaceful Atom

BY FRANCIS BELLO

The second new energy is the atom, as full of potential blessings as of destructive forces. In the next five years half a billion dollars will be spent by Business and the Atomic Energy Commission to bring nuclear power into our homes and factories.

Ten years, almost to the day, after the fantastic morning when the atom burst forth from behind a curtain of total secrecy, over sixty nations convened in Geneva to tell each other what they knew about peacetime uses of nuclear energy. Over thirty countries presented more than 1,000 papers, and while they did not reveal all, the big three—the U.S., Russia, and the U.K.—disclosed more about nuclear technology than anyone would have dared hope even a few months before. The International Conference on the Peaceful Uses of Atomic Energy, August 8–20, climaxed a swift series of events that clearly established 1955 as Year One of the Peacetime Atom.

The great fact of our times is that in the contest for world prestige, the ability to tame the atom has assumed incalculable significance. Since January 1, 1955, scarcely a day has gone by without a new atomic pronouncement from industry, the AEC, Congress, or the White House. "I think," said one thoughtful nuclear scientist, "we must be near the peak of the decibel curve. The curve of technological progress is, as usual, rising more slowly." Industry has never had a bandwagon to match atomic energy, and everyone, it seems, is battling to get aboard. There are already specialists in atomic-energy law, and there are atomic investment funds, atomic public-relations counselors, and atomic-information vendors galore.

But behind the noisy headlines, many firms are quietly making solid progress, and the progress is not all concerned with nuclear power. Nearly 1,000 companies have built "hot" labs and about 200 have become regular customers for the AEC's radioisotopes. These byproducts of atomic weaponry have given researchers a new tool of inquiry into the physical world at least as useful as the microscope. In addition, atomic radiations can influence chemical reactions, keep potatoes from spoiling, alter the qualities of plastics, and sterilize frankfurters. None of these applications has yet reached commercial scale, but any of them soon might. On all nonpower uses of atomic energy, industry plans to spend about $150 million through 1958. This estimate was recently published by the Atomic Industrial Forum, an atomic trade organization with about 300 members.

But no matter how provocative are nonpower applications, it is the atom as power source that dazzles the businessman—and the world. There seems to be a market for nuclear-power reactors as limitless as man's demand for energy. As of mid-1955, at least seventeen U.S. firms said they were prepared to build nuclear-power reactors, and the list is still growing.

Industry and the Atom: The Score in Mid-1955

If you are prepared for a complicated answer, just ask almost any large manufacturer if he is planning to build nuclear reactors. The firms that appear in the directory below have built reactors, have firm contracts for them, or are prepared to accept orders.

THE REACTOR BUILDERS: LARGE HOPES,

Companies offering reactors:	*Central station*
General Electric	Nuclear Power Group
Westinghouse	▲ AEC-Duquesne
American Machine & Foundry	Rural Coop. Power Assn.
Babcock & Wilcox	Con Edison
Fluor Corp.	Yankee Atomic
North American Aviation	Consumers P.P.D
Alco Products	
Allis-Chalmers	x
American Standard	x
Combustion Engineering	x
Curtiss-Wright	x
Foster Wheeler	x
General Dynamics	?
Walter Kidde Nuclear Labs.	x
Martin (Glenn L.)	
Nuclear Development (NDA)	
Sperry Rand (Ford Instr. Div.)	

SAR—Submarine advanced reactor
SFR—Submarine fleet reactor
LSR—Large ship reactor
APPR—Army package power reactor

Not included in the directory are a number of firms that will play an [illegible]

clear Metals.

FEW FIRM ORDERS

Package power	*Propulsion*	*Research*
x	▲ Seawolf: Mark A & B SAR, ANPP	x
x	▲ Nautilus: Mark I & II 2 SFR's, LSR	x
x	x	A.M.F.; ▲ Battelle
x	x	▲ U. of Mich.
x		
x		▲ Armour
▲ APPR		
x	x	x
x	x	x
x	x	
x	x	x
x	x	x
?	x	?
x		
x		
x	x	x
x		

ANPP—Aircraft nuclear power project
x—Prepared to take contracts

▲—Built or under construction
All others: under study

Understandably, the actual achievements of Year One are relatively puny. Just before the Geneva Conference opened, power generated by a nuclear reactor was fed into a commercial American transmission line for the first time. The power came from a General Electric-built prototype of the reactor for the country's second atomic submarine, the *Seawolf*. The reactor is at West Milton, New York, near Schenectady. The AEC generated the world's first electric power from the atom—enough to operate about a hundred toasters—in December, 1951, using a small experimental breeder reactor. Subsequently, as the U.S. concentrated on submarine propulsion, the Russians earned the distinction of building the world's first civilian nuclear-power station—a modest 5,000-kw unit that reportedly went into service in 1954.

Who will be first to the summit?

The big race, however, lies immediately ahead—the race to develop a large nuclear-power plant capable of producing power at a cost no greater than conventional fossil-fuel plants. Here lies the great opportunity for the AEC and U.S. industry working together. The Atomic Energy Act of 1954 seems to have unleashed, at last, the massive and diverse talents of U.S. industry. The pacification of the atom stands, like an Everest, as a towering challenge to the nations of the world. Russia, Britain, the U.S.—and others—will follow different routes to the summit. Each understands clearly that the winner will be hailed only if he makes the fruits of his victory quickly available to the rest of the world.

The obstacles to success are more than technological. The whole effort could be hobbled by a shortage of certain kinds of scientists and engineers. In both the U.S. and Britain those familiar with the problem are not optimistic that it will be easily solved. The effort could be just as seriously hobbled if the AEC failed to simplify soon the tangle of security regulations, including the recent setting up of a so-called "gray area," which has nearly everyone confused. One leading atomic consultant estimates that the ritual of con-

forming to security regulations takes up about one-third of his time. Gordon Dean, former AEC chairman, declares that if all reactor technology is not thrown open quickly, the Atoms for Peace program will simply fizzle out.

Beyond these matters, the U.S. faces a special handicap: conventional power in the U.S. is dirt cheap. Nuclear power will be competitive in many parts of the world long before it is competitive in the U.S. Yet before U.S. industry is prepared to build reactors for others on a major scale, it must acquire substantial experience at home. This means operating at home for many years in the red. The British, and perhaps the Russians, may be able relatively soon to operate at home in the black.

This fact was documented early in 1955 in a British White Paper entitled *A Programme of Nuclear Power*, which disclosed a government plan to build twelve large nuclear-power stations over the next ten years at a cost approaching $1 billion. (The program has since been expanded substantially.) Estimated cost of power from the first stations: about 7 mills per kwh, or about the same as power from new British coal-fired power stations.

In June, 1955, although the Russians disclosed no details, Georgi M. Malenkov, Minister for Electrical Power Stations, revealed that the Soviet Union would have a large nuclear-power plant—of 50,000 to 100,000 kw—in operation next year, or before the first British plant at Calder Hall. The British plant, also of 50,000 to 100,000 kw, had been, until then, advertised as the world's first big installation. The 60,000-plus-kw U.S. nuclear-power plant at Shippingport, Pennsylvania, jointly financed by the AEC and Duquesne Light Co., was to have been the world's second large plant. Now, it appears, Shippingport, scheduled for completion late in 1957, may come in third.

Six plants on drawing boards

Meanwhile, in mid-1955 the AEC had before it six nuclear-power-plant proposals from utilities—private and public:

SIX PROPOSED NUCLEAR-POWER PLANTS:

Sponsor	*Reactor type*	*Designer or builder*
Power Demonstration Reactor Program:		
CONSUMERS PUBLIC POWER DISTRICT, NEBRASKA	Sodium-graphite	North American Aviation
DETROIT EDISON GROUP: Central Hudson Gas & Electric Corp., Cincinnati Gas & Electric Corp., Consumers Power Co., Delaware Power & Light Co., Detroit Edison Co., Long Island Lighting Co., Philadelphia Electric Co., Rochester Gas & Electric Co., Toledo Edison Co., Potomac Electric Power Co., The Southern Co. (and subsidiaries)	Fast-breeder	Not yet decided
NUCLEAR POWER GROUP: American Gas & Electric Service Corp., Bechtel Corp., Commonwealth Edison Co., Pacific Gas [illegible]	Dual-cycle boiling-water	General Electric
[illegible] Maine Power Co., The Hartford Electric Light Co., The Connecticut Power Co., Western Massachusetts Electric Co., Public Service Co. of New Hampshire, Montaup Electric Co., New Bedford Gas & Edison Light Co., Cambridge Electric Light Co., Central Vermont Public Service Corp.	Pressurized-water	Monsanto Chemical Co. and Fluor Corp.
Other Proposals:		
CONSOLIDATED EDISON CO. OF N.Y.	Pressurized-water	Babcock & Wilcox
RURAL COOPERATIVE POWER ASSOCIATION (MINN.)	Closed-cycle boiling-water	American Machine & Foundry

Total

713,000 KW BY 1960?

Plant size (kw)	Estimated cost (in millions)	Cost per kw	Location	Probable operating date
75,000	$20–$25	$266+	Nebraska	1958
100,000	$54	$540	Michigan	1959
180,000	$45	$250	47 miles SW of Chicago	1960
100,000	$25–$30	$250+	Rowe, Mass.	1958
236,000	$55	$233	Buchanan, N.Y.	1960
22,000	$6	$273	Elk River, Minn.	1959
713,000	$205–$215			

Four of the proposals were made in response to an AEC request, in January, 1955, that industry take part in a Power Demonstration Reactor Program. As its contribution to the program the AEC offered: (1) to make a free loan of nuclear fuel until July 1, 1962, charging only for net fuel consumed, and for any processing performed on the spent fuel by the AEC; (2) to perform research and development work in AEC laboratories without charge; and (3) to pay in cash, in advance, for "technical and economic information" that may be acquired in the course of building and operating the nuclear-power plants.

The four proposals submitted in direct response to the AEC's invitation vary widely in details. Commonwealth Edison Co., Chicago, speaking for the Nuclear Power Group (itself and seven associates), said it was ready to go it alone with no subsidy or cash advance from the AEC. Taking the other three proposals as a group, the AEC estimates 80 to 90 per cent of the total cost would be borne by the utilities. The fifth proposer, Consolidated Edison of New York, simply asked for a license to build a nuclear-power plant. The Rural Cooperative Power Association at Elk River, Minnesota,

AEC's FIVE-YEAR REACTOR

Reactor type	*Designer*	*Heat output (kw)*
Pressurized-water reactor (PWR)	Westinghouse Electric Corp.	264,000
Experimental boiling-water reactor (EBWR)	Argonne National Lab.	20,000
Sodium reactor experiment (SRE)	North American Aviation	20,000
Homogeneous reactor experiment (HRE-2)	Oak Ridge National Lab.	10,000
Experimental breeder reactor (EBR-2)	Argonne National Lab.	62,500

which made the sixth proposal, said it was ready to contribute about $750,000 in existing facilities, and asked the AEC to build a power reactor, which the cooperative would operate and eventually purchase over a period of years. In all, the six plants would have a total capacity of 713,000 kw and would cost over $200 million. If begun promptly, the first might be running in 1958, the last by 1960.

Pilot plant vs. big plant

Concurrently with this power-demonstration reactor program, the AEC is financing, at an estimated cost of $250 million, a Five-year Reactor Development Program (see below), under which at least five different types of reactors are already under construction or in the planning stage. Except for the Shippingport PWR, all the units will be of pilot-plant size, i.e., just large enough to test principles and provide design data for future scaling up.

Why should industry build large nuclear-power plants—all frankly uneconomic—at the same time that similar and advanced reactor types are under development? Answer: normally, it shouldn't. But

DEVELOPMENT PROGRAM

Electric output (kw)	*Estimated cost*	*Location*	*Probable operating date*
60,000–100,000	$85 million *	Shippingport, Pa.	1957
5,000	$17 million	Lemont, Ill.	1956
7,500	$10 million †	Santa Susana, Calif.	1956
	$47 million	Oak Ridge, Tenn.	1956
	$40 million	Arco, Idaho	1958

* Includes $5 million from Duquesne Light Co.
† Includes $2.5 million from North American Aviation

there has never been anything normal about atomic energy, and its peacetime development will be no exception. It is as obvious to businessmen as to politicians that the world desperately wants the atom put to peaceful use. As the AEC has declared, "the early development of economically competitive nuclear power by the U.S. is an important national objective." It follows that anyone who promotes this objective may be obliged to make business decisions *that lack conventional economic justification.*

Utility executives, however, are not motivated solely by an urge to bring cheap nuclear power to the world. One of their chief reasons for acting, perhaps the most important one, is referred to only guardedly. The utility industry is mortally afraid of public power. "You know what happens," says one of the industry's leaders, "if you leave a vacuum. Something will fill it. We don't propose to leave an atomic-power vacuum."

Regardless of motives, the net effect of building big nuclear-power plants immediately is to gain experience quickly. This experience inevitably will be more expensive than if acquired at a more leisurely pace, but the U.S. seems willing to pay the price.

Who will pay?

Whatever the cost of nuclear power, it will be borne in the U.S. —as in Britain—by the public, one way or another. The British program will be wholly financed by the government. In the U.S. the bill will be passed on to the customer. Consolidated Edison, for example, can generate power in a modern fossil-fuel plant for about 7.5 mills per kwh, or just about the U.S. average. It estimates that its nuclear power will cost perhaps 9 mills, which is perhaps as reliable a figure as any quoted today. It is higher than the British figure of 7 mills, not because U.S. technology is inferior, but evidently because of differences in financing practices, in plutonium credits, and in construction and labor costs.

Fortunately, there are so many electric light and power cus-

tomers in the U.S. that the per capita cost of financing the power-demonstration program will be invisible. There will certainly be no rise in rates. Nevertheless, the six proposed plants will, if approved, cost perhaps $50 million over and above the cost of conventional plants of the same capacity. In addition, nuclear power from the six plants will cost, each year, about $10 million more than conventional power, if the nuclear is dearer by only 2 mills per kwh.

This money must come from somewhere. Probable source: over the next ten years it is predicted that the U.S. will have to double its electric-generating capacity (now 102 million kw). This will require scores of new and efficient fossil-fuel plants, which will produce power at a cost below the present average, and thus more than offset the higher price for a small amount of nuclear power.

The utilities' contribution will represent only a part of the investment needed to make nuclear power competitive. Other sizable contributions will be made by equipment manufacturers in the form of absorbed development costs. Assuming all the proposed nuclear-power plants get built, it is likely that over half a billion dollars in private and government funds will be poured into reactor technology in the next five years, with no guarantee that nuclear power will, at the end of the outlay, be economic.

Competitive power: when?

When *will* nuclear power compete in the U.S. with fossil fuels? The optimists say the second generation of power plants, going into service between 1962 and 1965, may be able to turn the trick by producing power in the 5-to-6-mill range.

At this rate, nuclear power would be as cheap as, or cheaper than, 68 per cent of the conventional power generated in large fossil-fuel plants built in the last ten years.

The AEC, in what it called an "optimistic" estimate, made in 1954, forecast that the atom would supply 10 per cent of all U.S.

there has never been anything normal about atomic energy, and its peacetime development will be no exception. It is as obvious to businessmen as to politicians that the world desperately wants the atom put to peaceful use. As the AEC has declared, "the early development of economically competitive nuclear power by the U.S. is an important national objective." It follows that anyone who promotes this objective may be obliged to make business decisions that lack conventional economic justification.

Utility executives, however, are not motivated solely by an urge to bring cheap nuclear power to the world. One of their chief reasons for acting, perhaps the most important one, is referred to only guardedly. The utility industry is mortally afraid of public power. "You know what happens," says one of the industry's leaders, "if you leave a vacuum. Something will fill it. We don't propose to leave an atomic-power vacuum."

Regardless of motives, the net effect of building big nuclear-power plants immediately is to gain experience quickly. This experience inevitably will be more expensive than if acquired at a more leisurely pace, but the U.S. seems willing to pay the price.

Who will pay?

Whatever the cost of nuclear power, it will be borne in the U.S. —as in Britain—by the public, one way or another. The British program will be wholly financed by the government. In the U.S. the bill will be passed on to the customer. Consolidated Edison, for example, can generate power in a modern fossil-fuel plant for about 7.5 mills per kwh, or just about the U.S. average. It estimates that its nuclear power will cost perhaps 9 mills, which is perhaps as reliable a figure as any quoted today. It is higher than the British figure of 7 mills, not because U.S. technology is inferior, but evidently because of differences in financing practices, in plutonium credits, and in construction and labor costs.

Fortunately, there are so many electric light and power cus-

ing a sodium-cooled reactor for the world's second atomic submarine, the *Seawolf*, (2) designing the SAR, or submarine advanced reactor, and (3) sharing in the effort to develop nuclear-powered aircraft.

In March, 1955, Commonwealth Edison announced that G.E. was prepared to build a so-called "dual-cycle" boiling-water reactor for the Nuclear Power Group's proposed plant. G.E. has offered to construct the complete plant, for $45 million, or $250 per kw. Commonwealth Edison, which will own and operate the plant, figures that a conventional installation of the same capacity would cost $30 million ($167 per kw), consequently it has agreed to contribute that sum toward the nuclear plant. It will also share part of the remaining $15 million of cost with the seven other companies that are in for the "experience." Since it is widely believed that the plant will actually cost much more than $45 million (perhaps as much as $60 million), it follows that G.E. is prepared to make a heavy contribution of its own.

Like Westinghouse, G.E. declines to indulge in the currently popular numbers game of predicting the kwh cost of nuclear power. But obviously G.E. believes its dual-cycle reactor will produce power at lower cost than any rival type at this stage of the art.

Who's No. 3?

Behind Westinghouse and G.E., four or five firms are jockeying for position, each with its own claim to eminence. North American Aviation, with more than 600 people in its Nuclear Engineering and Manufacturing Division, probably has the largest technical staff among the contenders. N.A.A. is also contributing $2,500,000 of the $10-million cost of the medium-sized sodium graphite reactor that it is building for the AEC in the Santa Susana Mountains, about 30 miles from Los Angeles. The AEC is now considering proposals to link this reactor to a 7,500-kw turbogenerator, which would make it the first nonmilitary U.S. reactor to produce energy for commercial use.

Nuclear Development Corp. of America, White Plains, New York, headed by a brilliant young nuclear engineer, John R. Menke, is another contender for third place. N.D.A. has contributed to the design of many of the important reactors built and proposed in the U.S. in the last seven years. Not content with being just a designer, N.D.A. recently bought 1,200 acres of land 40 miles north of New York City where it will experiment with reactor assemblies. Working now on aircraft-propulsion reactors, N.D.A. aspires to be a builder of other power reactors as well.

Another able organization is Walter Kidde Nuclear Laboratories, headed by Karl Cohen, a young scientist who made pioneer studies of the gaseous-diffusion process. With the support of the Walter Kidde organization—which is an engineering and construction firm as well as a maker of fire extinguishers—Kidde Nuclear is prepared to build power reactors up to 100,000 kw. "We were quoting $250 per kw when everyone else was saying $500," recalls Cohen. His firm's most recent quotation for a 75,000-kw plant: $216 per kw.

One of the sharpest contests for supremacy is taking place among the leading boilermakers: Babcock & Wilcox, Combustion Engineering, and Foster Wheeler. F. W. is ready to take orders for the aqueous homogeneous type of reactor, and C. E. is driving hard to win orders for propulsion reactors. But it is B. & W. that seems to be moving most swiftly.

Many nuclear engineers were surprised when B. & W. was selected by Con Edison to build the reactor for the proposed Indian Point plant. They were even more surprised at the announced cost of the complete plant: $55 million for 236,000 kw, or only $230 per kw.

Oil to boost the atom

There is, however, one important gimmick in the Con Edison plant design that helps to explain the low kw cost: only 140,000 kw will be provided by the reactor itself; the remaining 96,000 kw will be produced by an oil-fired superheater that will raise the tempera-

ture of the reactor-produced steam to 1,000°F., several hundred degrees beyond the steam temperatures that will be available at Shippingport or in G.E.'s dual-cycle plant. The high steam temperature will boost the heat efficiency of the Con Edison turbine plant from 26 to 31 per cent. Moreover, the oil-produced kw will be cheap compared to the nuclear. The stratagem is reminiscent of the early days of the steamship when shipbuilders, trying to have the best of both worlds, combined sails with steam propulsion.

In addition to its Con Edison project, B. & W. has been studying the fast-breeder reactor—for producing both power and high-grade plutonium—that Detroit Edison and ten associated utilities propose to use in their 100,000-kw power plant. No builder has yet been selected for this plant, but its design is being supervised by Atomic Power Development Associates, consisting of Detroit Edison and thirty-odd utilities, engineering firms, and manufacturers. Some experts no longer consider breeding so attractive as it seemed several years ago when uranium was scarce and plutonium more valuable than at present. Nevertheless, the Detroit Edison group believes the breeder deserves to be pushed.

Looking beyond the power plants of the two Edisons, B. & W. is growing enthusiastic about the liquid-metal fuel reactor, or LMFR, which it has been studying closely with Brookhaven scientists responsible for conceptual design.

Also Boston and Nebraska

For its power demonstration program, the AEC is also weighing the proposal of the Yankee Atomic Electric Co. (a group of twelve New England utilities), and another proposal by the Consumers Public Power District, a state-owned public-power agency with headquarters at Columbus, Nebraska.

Yankee proposed a 100,000-kw plant, using a pressurized-water reactor similar to that at Shippingport. Expected cost: between \$25 million and \$30 million. Present plans call for the reactor to be

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mental: it appears that long-cherished estimates of the country's coal supplies have been grievously in error. On the basis of 1909 geological inferences, it has long been accepted that the U.S. possesses coal reserves of about 3.1 trillion tons of all types, equivalent to a 3,400-year supply at past peak consumption rates of 600 million tons a year.

The vanishing coal pile

Recently, the U.S. Geological Survey has taken a hard look at coal and has concluded that the reserves recoverable at close to present mining costs are possibly only one-tenth the earlier estimates. These figures have been further scrutinized by Palmer Cosslett Putnam, in his remarkable book, *Energy in the Future*, prepared at the request of the AEC and published in 1953. Putnam measures energy reserves in terms of a helpful new quantity, Q. (One $Q = 1$ billion *billion* Btu. The total U.S. energy demand is now about 0.035 Q per year.) By this measure, the 3.1 trillion tons of coal—if they existed—would contain 67 Q. According to the best current estimates, the coal *actually* available in the U.S., at no more than twice 1950 costs, contains perhaps 6 to 8 Q. Putnam figures that natural gas, oil, and oil shale add only another 0.9 Q. He further estimates the world fossil-fuel reserve at 38 Q, of which coal is 32 Q.

Using plausible assumptions of population growth and a 3 per cent annual increase in per capita energy demand, Putnam suggests that peak production of coal in the eastern U.S. may be reached before 1975, and of all coal in the U.S. before 1990. From this he concludes: "*If we are to avoid the risk of seriously increased real unit costs of energy in the United States, then new low-cost sources should be ready to pick up much of the load by 1975 or sooner.*"

Only time will tell if Putnam is roughly correct. Meanwhile he has helped enormously to make the atom a sizzling business prospect.

One question remains: How are we fixed for uranium? Things

► But tonnage is only part of the story, overweighted as it is by steel's current, massive production, so huge as to hide all significant detail. If steel and the major light metals are plotted on a more realistic basis by volume, then a much more significant factor appears. In the last decade the light metals have moved up from a small to a ponderable percentage of steel's volume and, since they are growing at a much faster rate, they are going on, by projection through 1975, to challenge the primacy of steel itself.

► But volume, too, is only part of the story, for the most significant aspect of the last decade's explosion in metals is the variety of new metals coming up, small in volume but vital to a host of new industries and technologies. How fast they have grown is shown

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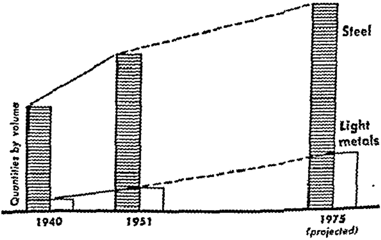
This chart shows the rough dimensions of the metals revolution. It compares steel with major light metals—alloy steels, aluminum, magnesium, and titanium—on the realistic basis of volume in cubic feet instead of tons. By projection, the light metals are shown rising on an upcurve through 1975 to 26 per cent of steel's volume.



by one fact: in 1941 the number of metals on the U.S. list of scarce and strategic materials was twelve; in 1951 the number was well over thirty.

In other words, we are at the real turning point of a great revolution. War and defense have had much to do with its speed, but its advance is implicit in the whole, complex technical civilization being built on this continent. While metals, starting from a much older and broader base, cannot show the phenomenal short-term growth of chemicals, they are exhibiting some of the same dynamics, the ability to create, or re-create, industries, new markets, new competition, new relationships in endless profusion. Since metals are, indeed, basic, this revolution is economic, political, and social, as well as technological, and its import is seen clearest in the perspective of history.

The ages of metals

From remotest time to the sixteenth century, men worked with but seven metals: gold, silver, copper, tin, iron, lead, and mercury. The next three centuries added seven more, antimony, zinc, bismuth, arsenic, nickel, cobalt, and manganese, of which two (bismuth and arsenic) were hardly considered metals at all. Up to 1900, with chromium and tungsten just coming into use, that was about the lot. About half a dozen major metals carried the wealth and weight of the world. Yet, in the next fifty years, the number of different metals known to commerce tripled, and not merely tripled, for with the development of high alloy steels and superalloys a broad stream of new materials became available. What caused this unprecedented upsurge and why was it so late in coming? The answers are important.

The old metals industry grew up in the fixed world of the ancients, secretive, prescientific. There were the noble metals and base metals of antiquity—those found closest to a pure state—and no more. Workable ore bodies were few. The great metal and

elements. Of the 100 elements now known, over three-fourths can be classed as metals.* Yet this large group has been the slowest to emerge. Of the eighty-three known metals, only about a third so far are of important tonnage or value, about a third are still of minor importance or just coming into use, and a third are practically unknown metals waiting future development. Thus nearly two-thirds of the area is still frontier. When it comes to putting a name to the era in which this frontier is being sweepingly entered, the familiar "Light Metals Age" won't do. It is more than light metals; it is a plethoric mixture of metals. For want of a better term, and provisionally, it is the New Metals Age.

The amazing metals mix

The new age has three leading characteristics. It is consuming metals, old and new, at a rate never before known in history. As the 1952 report of the President's Materials Policy Commission underlined, the U.S. alone since World War I has used most metals in quantities exceeding the total used throughout the entire world in all history preceding 1914. It is an age that, under this driving consumption and a second round of armament in a decade, is running into imbalances, temporary shortages, and sharply rising costs, particularly in the older, strategic metals. And, finally, it is the age of a multiplicity of metals, which in the long run offers the best solution to supply difficulties.

Under all these pressures, and with technology moving up to relieve them, steel, copper, and most of the older metals continue to grow bigger—steel embarking on a $500-million, low-grade taconite-ore program to hold its stake in the thinning Mesabi, while throwing out giant new supply lines to high-grade iron deposits in Labrador and Venezuela. It is plain that the time-tested older

* *The nonmetals: hydrogen, helium, carbon, nitrogen, oxygen, fluorine, neon, phosphorus, sulfur, chlorine, argon, bromine, krypton, iodine, xenon, astatine, and radon.*

metals are neither going to disappear nor much diminish for a long time to come. But the new metals grow faster. And while each metal has its own character, and many are irreplaceable in certain uses, the sheer variety of new metals introduces an explosive new freedom of choice and action.

It is this power of technology to create new materials and techniques on an ever widening scale that injects a new element into twentieth-century economics, confounding both Marx and Manchester. Reasoning from the still fairly static materials of nineteenth-century industrialism, it was easy to foresee a concentration of power in monopolies, oppressive or shining, depending on the viewpoint. But, while the concentrations of power have taken place, technology is now constantly creating new situations, shifts in power, and a new kind of competition that make old monopolistic concepts less and less operative. This is nowhere more clear than in the metals, where in the upthrust of new materials, markets are in the greatest flux in history.

The real force of this new order may be seen in a development that, in the last few years, has made a new material out of the oldest of all ferrous products, dormant and declining for a century—cast iron. International Nickel's laboratories, searching for improvements in nickel-alloy cast irons, developed a method for making brittle cast iron as strong and tough as ordinary steel by small additions of magnesium. Called ductile or spheroidal graphite iron, it was an entirely new material—strong, ductile, wear-resistant, heat-resistant—combining the casting economy of iron with many of the advantages of steel.

Few metal developments have moved so quickly as ductile iron. Opened to licensing in 1949, the process had, within three years, over 200 licensees in eighteen foreign countries and the U.S., operating in over 600 foundries. It is replacing gray iron, cast steel, wrought steel, bronze, and even aluminum in big diesel and automotive crankshafts, gears, bearings, plowshares, pistons,

compressor heads, valves, pipes, and fittings. And, through precision casting by the shell-molding process, it is opening up for foundries a whole, new, low-cost field of precision-parts manufacture. It is, in fact, putting the foundry industry, with over 3,000 small, dispersed units, back in competition with big steel and big steel fabricators.

The shifting mix

Across the board, the metals shift and strain as developments rise and the pressures vary. Aluminum takes an increasingly substantial bite into copper's wire and cable markets, and bites steadily into other copper uses. The can industry pushes coatings on steel to replace perennially critical tin in 80 per cent of its production, while a new aluminum solder moves in to displace tin alloy in smoothing automobile bodies. And the Malayan tin industry fights back with increased research and assurances of ample supply. Union Carbide's Electromet division makes a notable development of manganese-containing stainless steel to cut in half the content of critical nickel, urgently needed elsewhere. Titanium alloys begin to displace stainless steel in aircraft. And continuous casting and hot extrusion of stainless bring it within range of competing with aluminum for other markets. General Motors develops an aluminum dip for coating steel to replace zinc, which is also being squeezed out of increasing numbers of automotive parts by aluminum die castings. And moving up behind aluminum are magnesium die castings, propelled by the economics of getting more parts per pound from a metal lighter than aluminum.

No small part of the flux is economic, in which regard the older metals industry is increasingly vulnerable as it is forced to thinner ores, deeper mining, more distant ore bodies, with costs mounting all along the way. Over a 1939 base, zinc prices had by 1952 soared 280 per cent, lead 275 per cent, pig iron 145 per cent, copper 118 per cent, while the higher-priced newer metals as a whole had been

held to the well-managed price levels of the chemical industry, 25 per cent above 1939, with aluminum actually 5 per cent below 1939. Unless the older metals companies attack their costs, diversify, and more aggressively develop new markets, the competitive gap between old and new will close tighter. And not only under the squeeze of new metals, for the chemical industry is ambidextrous and catholic in developing new materials. Polystyrene plastics already have ousted some 9,000 tons a year of aluminum and 2,000 tons of stainless steel in making refrigerators. And polyester-glass-fiber laminates are coming up fast as a remarkable new engineering material, making its bow in all-plastic, stock, sports-car bodies. For the next quarter of a century at least, this amazing materials mix will be precipitating a new industrial structure.

The light metals

To see some of the major directions in which this structure is building, it is necessary to review the new metals by groups and functions. The first group in point of size, dynamics, and growth is the structural light metals, in which the signal fact is the rapid rise to tonnage proportions.

[illegible] hese [illegible] els— [illegible] into [illegible] ment, and other high-technology industries. Now about 10 per cent of total steel, [illegible]

[illegible] Ohio, [illegible] world's largest, with two radically new mass-production [illegible] for low-carbon ferrochrome and electrolytic chrome and [illegible]

nese. Expanding in scale below Electromet is a tight, competitive group of less-diversified alloy suppliers, including Pittsburgh Metallurgical, Ohio Ferro-Alloys, Vanadium Corp. of America, and Climax Molybdenum.

Aluminum is now running at five times its 1940 rate of production. Yet the industry consensus is that aluminum has only begun to realize its potential, so far primarily based on aircraft and transportation but beginning to exploit the building, construction, electrical, automotive, appliance, and air-conditioning industries. Versatile, still-developing alloys will open still other fields. Only since 1945 the number of aluminum fabricators has jumped from 5,500 to nearly 20,000.

Magnesium is moving at more than twenty times 1940 production. The lightest of structural metals, magnesium had the disadvantage of following closely on aluminum in time and end uses, with some peculiarly tough fabricating and corrosion problems of its own. But in 1951 magnesium registered a significant growth stage when its wrought products passed castings in volume; and increasing markets in the automotive, trailer-truck, materials-handling, machinery, and appliance fields, as well as in aircraft and ordnance, indicate that the metal has reached a major turning point. In the long view, moreover, magnesium's limitless availability, lightness, and strength in evolving alloys must lead it to parallel aluminum.

Titanium, almost non-existent as a pure metal a few years ago, is on the most spectacular growth curve of all, up from nothing to 1,200 tons in 1952. Defense goals for 1955 were hiked twice in twelve months, first to 10,000 tons, then to 22,000. In contrast to aluminum's or magnesium's quarter-century struggle to reach like tonnages, this is the fastest development in history of an entirely new metal. It is the more phenomenal in that the problems besetting titanium make those of the older light metals pale. A highly reactive metal in the molten state, ductile titanium is

wrested from ilmenite and rutile-ore sands only by the most laborious means. The only commercial methods thus far are based on the U.S. Bureau of Mines' cumbersome Kroll process, by which the metal is batch-handled in inert helium atmospheres and vacuum-arc furnaces, producing a mill-finished product at $15 to $30 a pound. Yet titanium has such a combination of high strength, light weight, and good heat and corrosion resistance—bridging the gap between aluminum and stainless steel—that even at the price it is a natural for high-speed aircraft, naval construction, and ordnance.

Getting the new metal into production, while simultaneously solving its multiple alloying and fabricating problems, has generated a scramble unlike any previously known in the metals. Nearly everyone has going a research project to outflank the Kroll process, including Union Carbide, Monsanto, Dow, National Research, Horizons, Inc., Glidden, Kennecott Copper, New Jersey Zinc. To encourage the market and build experience, the Air Force has now specified titanium sheet and parts in two advanced jet planes. Originally overglamorized as a metal to withstand high jet-engine heats, titanium is now more soberly seen as a major addition to the structural metals, important not only to the military but to many prospective industrial users.

The rare metals

Ranging far below the light metals in volume, but wider in variety of use and outlook, is a growing group of materials that might be called the rare metals. Except for such older items as platinum, palladium, and indium, these are, for the most part, rising new metals of specialized uses that may grow to considerable poundage but never perform more than small, critical functions in advancing technologies. A few, however, may someday soar out of the rare category on some turn of events, for this group forms part of the metals frontier.

Perhaps the member of this group closest to the light metals is titanium's sister metal in ductile form, zirconium, which has many of the same properties as titanium, is obtained by the Kroll process, but is more difficult and expensive to get into pure ingot form. Thus far its only outstanding property is high permeability to slow neutrons, a factor useful in nuclear-reactor construction; hence the Atomic Energy Commission has been pushing production. Only two producers have made zirconium in pure bar form, Foote Mineral Co. and Westinghouse, working up a laboratory process to industrial scale to produce metal to the fantastic purity of 99.7 per cent. The program for more basic production now includes a new $2,400,000 plant operated by the Carborundum Metals Co. for the AEC at Akron, New York, to supply sponge zirconium and hafnium, a metal closely associated with zirconium in its ores and in its properties. Until secondary or civilian uses appear for the metal, however, private producers are reluctant to risk their own money on zirconium metal. Less pure or alloying forms have been made for some time by Carbide's Electromet, National Lead, and others, for deoxidizing high-alloy steels and for creating a growing number of nonferrous alloys. A new zirconium-cerium alloy of magnesium so raises strength and stability at medium temperatures that it has put magnesium castings into some jet-engine parts. More aggressive development of high-purity zirconium alloys will probably produce special, high-corrosion-resistant materials and a possible replacement for scarce tantalum.

The work on titanium and zirconium has stimulated much work on getting other metals into highly pure forms, for with titanium it was discovered that small impurities could change the entire character of a metal. Inclusions of oxygen and nitrogen beyond 0.2 per cent, along with other slight contaminations of carbon, silicon, aluminum, or iron, made all the difference between getting ductile metal and a useless, brittle composition. Union Carbide, Climax Molybdenum, Foote Mineral, Battelle, and others have been work-

ing in turn on ultrapure chromium, vanadium, molybdenum, columbium, tungsten, silicon, calcium, manganese, lithium, boron, and hafnium. In pure form these are to be classed as rare metals, with characteristics quite different from previously known forms. They are just beginning to develop uses and are likely to lead to a whole range of new alloys.

The range of the rare

The range of rising new metals is bewildering. There is germanium, the semiconductor or electronic-transistor metal, which is moving so fast that dozens of small manufacturers, trained by Bell Labs and others, are springing up to meet demand, and West Virginia's coal beds are being ransacked for germanium-bearing seams to extend the supply. Zinc concentrates are the only U.S. source thus far, and Eagle-Picher Co. is the main supplier. But there are also other developing semiconductors: silicon metal, selenium, tellurium, gray tin, and a new cadmium sulfide crystal that may equal germanium in efficiency.

And there is silicon metal itself, which is going into the big growth of silicone plastics, and moving into the rising field of intermetallic coatings on metal. And lithium, the lightest metal known, but reactive with air and water, which goes into lithium-silicone high-temperature greases, new magnesium alloys, and aluminum welding and brazing compositions, and which may become the main source of tritium, or heavy hydrogen for the hydrogen bomb. Lithium compounds are in expansion, mainly by Lithium Corp. and Foote Mineral.

Then there is gallium, a metal that melts in the hand, and expands like water on freezing, whose unusual properties are being explored on the basis of a few hundred pounds' production by Eagle-Picher and Alcoa. And there is a weird, important, new combination of bismuth and manganese called Bismanol, developed by powder metallurgy in the Naval Ordnance Laboratory. This

material, though both constituents are nonmagnetic, forms a permanent magnet with the highest coercive force known, aimed to supplant high-nickel-cobalt Alnico magnets in many uses.

Finally, just about to come into much wider exploitation are the rare-earth metals, a group of fifteen or so metals with fantastic names, which are neither very rare nor earths, but which are so closely mixed in their ores that most of them until recently have resisted separation in any quantity. Cerium, the best known of the group, has been available for a long time in a mixture of rare earths more properly known as misch metal, which makes lighter flints, and is growing as an alloy in magnesium and in stainless and other steels, where it adds workability. But development under the AEC of ion-exchange extraction and a new liquid-to-liquid industrial separation process is now bringing out in kilogram amounts for the first time relatively pure rare earths: in order, lanthanum, cerium, praseodymium, neodymium, promethium, samarium, europium, gadolinium, terbium, yttrium, dysprosium, holmium, erbium, thulium, ytterbium, lutecium. And discovery of large, new rare-earth ore bodies in the West has increased activity. This is a vast area of unknown properties and uses to be explored. But one of the metals, gadolinium, has the highest known absorption of slow neutrons, which makes it potential atomic-reactor material, and there is likely to be found in the group a range of entirely new light-metal alloys and additives to steel and super-alloys.

The hot metals

The next large group, the refractory metals, embraces the most pressing and intense area of development on the metals frontier. The salient point of this frontier is the bladed turbine wheel of the aircraft gas-turbine or jet engine. Under the blasting hot temperatures and centrifugal loads and stresses at which these blades must operate, they are pressing the ultimate limits of presently known metals. The thin turbine blades must maintain their strength,

curvature, shock resistance, and dimensional stability in the red-hot range of 1,300° to 1,600°F. and beyond. Steel in this whirling inferno would burn out like paper. Titanium, whose high melting point (3,300°F.) had built high hopes for a great heat-resistant metal, has so far developed no alloy that does not begin to lose strength sharply around 1,000°F. Stainless steel, a ranking high-temperature metal, starts to lose its grip rapidly at 1,300°F. Yet the jet engineer, to gain the efficiencies inherent in the thermodynamics of his engine, is pressing for metals or materials to break the heat barrier at 1,600° to 2,000°F.

So far the only working metals approaching this barrier are a group of complex nickel-chrome-cobalt-molybdenum-columbium-tungsten super-alloys, containing little or no iron. These are related to or descended from the old Stellite cutting-tool alloys developed by Haynes Stellite Co., another division of Union Carbide, which underlies most of U.S. metallurgy, and which is the major supplier of super-alloys to the hottest spots in jet aircraft. Also within the super-alloy range, but in limited use in this country, is a British group of nickel-chrome alloys, minus cobalt, called the Nimonic series, which bears the brunt of British jet production. The newest Haynes cobalt-base, high-temperature alloy, however, has reached an operational level of 1,600°F., and for short intervals, 1,800°, which is the ultimate thus far.

The working and shaping of such refractory metals has the banging, brawny air of a frontier. Only the oldest, heaviest type of mills can work these red-hot hard metals, which give forth deep groans and crunching protests in the working. The critical turbine blades are either forged or cast, and a big producer like Thompson Products does both. Haynes Stellite has adapted to bucket-blade mass production the "lost wax" or investment-molding method of precision casting, ancient to the jewelry and dental arts.

13
The Transistor

BY FRANCIS BELLO

If we are living in the pushbutton age, what lies behind the button? The transistor, only the size of a pea, deserves this chapter to itself, for it has the potential to revolutionize the great electronics industry, not to mention the way America lives.

For nearly five years the electronics industry lived with a pea-sized time bomb called a transistor, a device that can replace vacuum tubes for many jobs. Then came 1953, the explosive year when the transistor went into volume production and the vacuum tube, unchallenged for nearly half a century, passed over to the defensive. Since the electronics industry works under high competitive pressures and enjoys matchless manufacturing flexibility, the transistor was in a position to move swiftly into an important fraction of the industry's products. More important, the transistor promised, to a large degree, to free electronics from the vacuum tube's onerous limitations—relatively short life, high power consumption, bulkiness, and fragility.

Just as the jet engine represents a whole family of reaction motors, and nylon a growing family of synthetic fibers, so the transistor heads a new class of electronically active solids. The industry has used such solids for years, but usually in conjunction with evacuated or gas-filled tubes (e.g., the phosphors in fluorescent lamps and television picture tubes; the cathode coatings in ordinary vacuum tubes). The transistor symbolizes the new electronics in which electrons go about their work, with a minimum of heat or fuss, without ever leaving the solid state.

The active bit of solid substance in a transistor is barely visible to the naked eye; yet the transistor and its minute relatives will almost certainly stimulate greater changes in commerce and industry than will reaction motors, synthetic fibers, or even, perhaps, atomic energy. Reason: the new solid-state devices will provide the reliability, compactness, and low power consumption needed to lift information-handling and computing machines—the nub of the second industrial revolution now upon us—to any imaginable degree of complexity. In the transistor and the new solid-state electronics, man may hope to find a brain to match atomic energy's muscle.

It was in 1948 that physicists at Bell Telephone Laboratories were running crucial experiments with germanium that led to the announcement of the transistor, the first practical solid device that, like a vacuum tube, could amplify an electric signal. Five years later, in 1953, engineers of Western Electric, Bell Labs' manufacturing affiliate, were getting ready to set in motion machinery to put at least a dozen varieties of transistors into mass production. As the year opened, the production rate of Western Electric, Raytheon, R.C.A., G.E., and others, combined, was probably under 50,000 units a month. The plant that Western Electric was equipping at Laureldale, Pennsylvania, was to have a monthly capacity of 1 *million* transistors and related devices. (U.S. vacuum-tube production is about 35 million per month, of which the largest

producer, R.C.A., may make about one-quarter.) The Laureldale facilities and pilot lines at four other locations (operated by G.E., Raytheon, R.C.A., and Sylvania) were being financed by the Army Signal Corps on behalf of the Department of Defense, at a cost approaching $13 million.

Understatement, solid-state division

By prevailing publicity standards, the transistor received one of the most restrained send-offs in recent memory. The New York *Times* of July 1, 1948, carried the big news on page 46, buried at the end of a column of radio chitchat. The import of the announcement was not lost, however, on commercial tube makers, who had heard rumors of the device. When Bell continued to refer to its bombshell with rare circumspection, panic was allayed. In fact, the tube makers' initial consternation soon turned to frustration as each tried to get sample transistors out to its customers, only to run into one wall after another.

The original device announced by Bell Labs was the so-called "point-contact" transistor. In mid-1951 Bell disclosed a second major development, the junction transistor, in some respects an improvement over the first device. For a considerable period pilot-production lines operated by Western Electric were the chief source of both types used experimentally by the armed forces.

In a couple of years, when production figures take on more meaning, no one should be surprised if the leading transistor makers turn out to be today's leading tube makers: R.C.A., G.E., Raytheon, and Sylvania. (Western Electric will continue to produce primarily for its own and military needs.) The big companies have been loath to release transistors until they were reasonably sure the product was reproducible and free from performance quirks. "Once we put transistors in our catalogue," says one of the big tube makers, "we're saying we can duplicate that device from

here out, and heaven help us if they start going sour after a few years of service."

No fewer than twenty-five U.S. firms and ten foreign (chiefly NATO) manufacturers have taken Bell licenses with the intention of getting into the transistor business. (In addition, R.C.A. licensees, which number over 300, may make transistors under existing R.C.A.-Bell agreements.) Much of the widespread interest in the transistor can be credited to the wise government decision, urged by Bell scientists, that secrecy would hurt the U.S., by stifling free exchange of ideas, more than it would hamper the Russians. To convey transistor theory and technology to licensees and others, Bell has given three intensive courses, five to eight days in length, in addition to publishing dozens of papers and providing speakers for scores of lectures. Only manufacturing techniques have been withheld from the public, and even they are only in the mild "restricted" category. "The transistor development," says Ralph Bown, research vice president of Bell Labs, "indicates the depth and power of present-day technology. The vast build-up and research being put into the transistor, in advance of any commercial return, reflects industry's faith that scientists and engineers know what they are talking about."

Early in 1953 the first small commercial returns began coming in. Toward the end of 1952, the transistor made its debut in a commercial product—the Sonotone hearing aid. Sonotone announced that in its new $229.50 instrument, one of three vacuum tubes had been replaced by a transistor, permitting "double the power at half the operating cost." Within a matter of weeks over fifteen hearing-aid manufacturers boasted they were using transistors. Thus, only eighteen months after the junction transistor was announced, it was beginning to supplant vacuum tubes that had enjoyed forty-five years of steady refinement. To be sure, the first transistors used in hearing aids cost perhaps $5 to $8 each, as

against $1 to $1.60 for a subminiature tube, but the game is still young.

Military godsend

Western Electric's 1953 million-a-month transistor crash program was designed solely to meet military needs. The military Research and Development Board recognized in the fall of 1951, *shortly after the announcement of the junction transistor, that the* transistor was too vital to wait for firm demonstration of military usefulness before creating production facilities. Hence the program, modeled after the wartime atomic project, went ahead with application and plant construction simultaneously.

In 1952, to study applications, the three services could obtain only about 90,000 transistors, mostly point contacts. Since the Air Force spends a significantly higher fraction of its equipment dollar for electronics than do the other services, it will probably find the most work for the transistor to do. About 25 per cent of the cost of modern combat aircraft goes into electronics, and in some cases the figure is over 50 per cent. Electronic devices may perform as many as thirty functions in aircraft, compared to three or four at the start of World War II.

A survey made by the Air Force in 1952 disclosed that, allowing for practical difficulties, "transistorization would be technically sound at present" for about 40 per cent of the jobs performed by vacuum tubes in airborne navigation, fire control, and radar. (It is never possible to pull a tube out of a socket and substitute a transistor; a new circuit must be designed from scratch.) The report concluded that with the then existing transistors and components (which can now be miniaturized to match the transistor), airborne equipment could be cut one-fifth in size, one-fourth in weight, and that failures would be slashed 40 per cent. The last figure evidently assumes no transistor failures in the life of the

equipment.* The Air Force finds that ordinary vacuum tubes have an airborne life expectancy of only 100 to 500 hours.

Computers in shoe boxes

It is possible that the Army, and particularly the Signal Corps, will benefit from the transistor nearly as much as the Air Force. An armored division, for example, now carries 120,000 operative vacuum tubes, four spares for every socket, and 150 electronics-maintenance men. While the Signal Corps is doing considerable transistorizing, top priority is going to wholly new devices never considered practical with vacuum tubes.

Item. In Korea, as in World War II, most casualties were caused by enemy mortars and small-arms fire. By one method of locating their source, the Army tediously plotted information phoned or radioed in by spotters. The Signal Corps is at work on a "shoe-box-size computer" that will do the job faster and more precisely than men ever could.

Item. A "Telesynd" group in the Signal Corps is engineering a low-error communication system for transmitting complex intelligence long distances over ordinary telephone circuits. The information transmitted is that which ordinarily passes between radar spotting devices and fire-control radars. A Telesynd transmitter and receiver built with vacuum tubes together weigh 1,400 pounds and consume 2 kw of power. On Signal Corps contract Bell Labs has gone far enough to demonstrate that transistorized equipment should require only about one-tenth the power and weigh less than one-fifth as much.

With their present limitations, transistors will find their greatest value in computers and in communication systems whose frequen-

* *Bell Labs actually does not know how long transistors will last. Transistors made in 1950, by immature techniques, have been on test long enough to indicate an average life of about 75,000 hours. For more recent transistors Bell says experience is too short to justify an extrapolation.*

cies are low enough to be handled by wire. Whereas vacuum tubes operate up to 10,000 megacycles and beyond, it is difficult to make transistors that will operate much above 150 mc. (Ordinary TV frequencies are around 100 mc; the new UHF channels range up to 890 mc.) Transistor experts, however, recognize no theoretical limitations on frequency, and point out that the transistor already goes to higher frequencies than tubes did at a comparable stage of development. They are equally optimistic about overcoming the transistor's other limitations—i.e., somewhat higher noise than tubes, and loss of reliability above 175°F. The transistor is so young that difficulties that loom large today will undoubtedly end up as footnotes in the future history of the development.

Transistors in the home?

The transistor could right now, in theory, take over the work of 90 per cent of the 1 billion or so vacuum tubes in U.S. radios (114 million) and television sets (22 million). In 1952, when R.C.A. showed its licensees over twenty experimental applications for transistors, including radios, phonographs, and other sound-handling devices, even some of R.C.A.'s competitors were astonished. The hit of the show was a portable TV receiver completely transistorized except for the picture tube.

"Our transistor symposium," says Dr. C. B. Jolliffe, vice president and technical director of R.C.A., "appears to have generated greater excitement in the industry than any technical presentation we have ever made, including color TV. We have been flooded with follow-up requests for information." R.C.A. feels sure that the transistor will be propelled into home instruments well before its use can be justified by price alone. Reason: it will simplify circuits and lead to important manufacturing economies.

Even without the home instruments market, there will evidently be eager customers for every transistor produced. Automobile manufacturers suspect that a variation of the transistor, the photo-

transistor, may soon replace the photomultiplier tube for operating automatic headlight dimmers. International Business Machines and others are testing transistors in computers that may find their way into both plant and office within the near future. The transistor should finally dispel the notion that in the automatic factory electronics-maintenance men might outnumber the workers in present-day plants.

Transistors for telephones

No one has greater need for the transistor than the Bell System. Says Ralph Bown, research vice president of Bell Labs: "One half of the total telephone plant, the local half, is now built—of necessity—substantially without vacuum tubes. This is where transistors will come in. The transistor requires less than 1/100,000th the power required by a vacuum tube; hence we can afford to use it in places and in quantities we could never use vacuum tubes. The transistor gives us the tool to do the things that the future of our business will require."

One illustration will indicate what Dr. Bown has in mind: An automatic-dial office of average size contains about 65,000 mechanical relays that connect one phone with another in response to the number dialed by a customer. The relays, which can perform slightly less than 1,000 switching operations per second, limit the number of calls that can be handled at any one time. Vacuum tubes might easily perform a million switchings per second, but their excessive power demand, size, and over-all cost are too high a price to pay. Transistors, which can switch as fast as vacuum tubes, do not have the tube's drawbacks. Moreover, Bell Labs has no doubt that most of them can be built to last for the telephone office's engineered life, i.e., forty years.

But even the transistor cannot "revolutionize" the telephone business. A $12-billion plant investment can be transformed only at an evolutionary rate. The best place to use the transistor immedi-

ately is in a new device that will extend the capability of the present system, and that is just what Bell is doing. Bell's first important use of the transistor is in a device called a card translator. Its job is to provide quick and automatic selection of routes for long-distance phone calls. Inside the device are as many as 1,000 metal cards, about 5 inches by 7½ inches, standing like books on a shelf. Each card (representing the name of the called exchange and its geographic area) carries a distinctive code perforation and can be jiggled up or down magnetically. A light shining through the card stack emerges at the far end in a pattern dictated by the up or down position of the cards. This pattern, read by a grid of phototransistors and amplified by transistors, provides the key to available intercity circuits. The card translator will select called routes in less than a half second.

The transistor's family tree

"The transistor," says Ralph Bown, "certainly ranks as one of our most fortunate strokes." When Bell Laboratories announced that a tiny piece of germanium, with a wire on the bottom and two "cat whiskers" on top, would amplify an electric signal, dozens of physicists in other laboratories felt that with luck they might have discovered the same effect. Some could hardly believe it was anything but an accidental discovery.

Most of them now know better. The transistor was as close to a deliberate invention as any such thing can be, and it underscores once again the absolute importance of fundamental research. The history of the invention briefly was this:

From the earliest days of radio it had been known that certain natural crystals, notably galena (lead sulfide), had the ability to transform an alternating current into a direct one. This is called rectification and is essential to radio reception. A piece of galena with a "catwhisker" probe (technically a crystal diode) became a standard part of early radio sets.

Galena rectifies because it falls into the class of materials known as semiconductors. In the Twenties the electronics industry fastened on to the vacuum tube, which could not only rectify but amplify, and forgot all about the odd substances that fall midway between insulators and highly conductive metals. The fundamental explanation why some substances conduct and others do not had to await the elaboration of quantum mechanics by some of the most brilliant theoretical physicists of the last half century: Niels Bohr, Louis de Broglie, Erwin Schrödinger, Werner Heisenberg, Wolfgang Pauli, and Paul Dirac. Important details were filled in by Arnold Sommerfeld, Enrico Fermi, Frederick Seitz, A. H. Wilson, Eugene Wigner, and others.

As the Thirties opened, very few industrial laboratories saw any profit in quantum mechanics or in what avantgardists were beginning to call the "solid state." Bell Laboratories was among the few, and its scientists began experimenting with semiconductors in the light of the new theoretical understanding. These studies led Bell and others to develop one extremely useful solid-state device, the thermistor (thermally sensitive resistor), in which resistance *decreased* with rising temperature. In ordinary electric circuits resistance *increases* with temperature. The thermistor proved invaluable in compensating for temperature variations encountered by cross-continental phone lines. Bell also worked out one or two other solid state devices, and by 1940 such devices about matched the number of vacuum tubes in the telephone system.

Solid state key to radar

During World War II scientists found that vacuum tubes could not rectify extremely high-frequency radar signals, so they turned once again to crystal diodes. This time the crystal was not galena, but silicon or germanium. Following the war, crystal diodes were indispensable in development of microwave relays and found their way into television receivers and into computers. (The UNIVAC,

sophisticated descendant of the 18,000-tube ENIAC, contains over 18,000 diodes and only 5,400 tubes.) G.E., the largest producer of germanium diodes, expected to make about 10 million in 1953.

Superficially a transistor is a diode (a two-wire device) to which a third wire is attached, but to create such a device that would actually amplify required new physical insights. The man chiefly responsible for obtaining these insights was William Shockley. In 1939, then a twenty-nine-year-old Ph.D. from M.I.T. and a member of Bell Labs' physical research staff, Shockley began trying to figure out ways to make a semiconductor amplify.

It is not often that the events leading up to a major invention can be described so simply as in the case of the transistor. Between 1940 and 1945 William Shockley had to drop his quest for solid-state amplification to work on war problems. "It was shortly after the war," he recalls, "when I cooked up a possible form of solid-state amplifier on paper. When we tried it, however, we got negligible results even though we could calculate what results we should get. John Bardeen [a member of Shockley's group] proposed a theory involving surface traps to explain why the device didn't work. You see, we started with a device idea, then got a physics idea. This led to more physics experiments to learn more about surface states."

Two and one make three

The team that conducted the surface-state experiments consisted of John Bardeen, a theoretical physicist (now at the University of Illinois), and W. H. Brattain, an experimental physicist who had worked on semiconductors during the Thirties. The key experiment involved a germanium diode immersed in an electrolyte (i.e., a solution that conducts electricity) that was connected to a source of direct current. This, in the words of Bardeen and Brattain, "led to the concept that a portion of the current was being carried by

holes flowing near the surface. Upon replacing the electrolyte with a metal contact, transistor action was discovered."

The transistor first announced in June, 1948, thus consisted of a small slice of germanium carrying two fine wires on top and a heavier wire soldered underneath. This was called a "point-contact" transistor. Simultaneously, Shockley had worked out the design of another possible amplifying device, which he called a junction transistor. It proved originally far more difficult to make than the point-contact type, but a working model, built by Morgan Sparks and Gordon Teal, was demonstrated by Bell Labs in June, 1951. The junction provides greater amplification and requires less power than the point contact, but it is not necessarily better for every purpose.

Bell Labs could not foresee how many fractious problems would have to be solved before the two types of transistors, so simple in concept, could be put into production. The man who has carried the heavy development responsibility is Jack A. Morton, the electrical engineer who devised the microwave transmitting tube that relays television signals from coast to coast. In all, Bell has about 100 scientists and engineers working on transistor theory and development. In addition, almost as many Bell and Western Electric engineers are working on production methods.

Today a laboratory without a special "solid-state" research group is mean indeed. The transistor has inspired a vigorous re-examination of many solid-state phenomena that are still imperfectly understood. These include: thermionic emission (source of electrons in ordinary vacuum tubes); photomission (key process in standard TV camera tubes); luminescence (source of light in TV picture tubes and in fluorescent lamps), photoconduction (source of TV signal in the Vidicon tube used in R.C.A.'s midget "Walkie-Lookie" camera; also the basis of a new photographic process called xerography).

Of the group, luminescence, particularly in fluorescent lamps, has been brought to the highest state of efficiency. Recently Sylvania, one of the leading lampmakers, has been working on a method for taking fluorescent lighting out of glass tubes. In its so-called "Panelescent" system, phosphors are sandwiched between glass sheets. While the sandwich does not yet shine so brightly as conventional fluorescent lamps, Sylvania hints it will soon make its appearance in a commercial product.

Photoconduction is perhaps the least understood of the four processes and one to which R.C.A. is looking hopefully for valuable new applications. The Vidicon pickup tube, for example, promises to operate with even less light than R.C.A.'s famous image orthicon, the standard television tube today.

Two other solid-state phenomena are also receiving much attention. They are ferromagnetism, perhaps the first solid-state phenomenon recognized by man, and ferroelectricity, a rather new subject. (A ferroelectric substance stores electric energy directly, much as a ferromagnetic substance stores magnetism.) The biggest recent advance in ferromagnetism was the development by Philips (the Dutch firm) of ferromagnetic spinels—ceramic-like magnets that, unlike ordinary magnets, do not conduct electricity readily. R.C.A. has high hopes that the spinels will crack the problem of providing electronic computers with nearly instantaneous memory units.

Ferroelectrics, usually ceramics, have recently found practical use in memory devices. One such device announced by Bell Labs, although still in early development, has a far higher storage capacity per square inch than R.C.A.'s spinel memory. The Bell memory crystal (barium titanate) can store 2,500 energy dots, or bits of information, per square inch.

The intense research effort going into the solid state is without industrial precedent. R.C.A., for example, has over a quarter of its 300-man research staff at work on solids; Bell Labs has concentrated about one-eighth of its 2,300 researchers in the same area.

While the chemical industry may be putting somewhat more research money into plastics, the money probably does not exceed a conservative 3 per cent of sales.

The research money being spent on the solid state is to an extraordinary degree gambling money. Present sales of strictly solid-state electronic devices are relatively insignificant. The inescapable conclusion: industry expects the solid state to pay off handsomely.

March, 1953

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The Automatic Factory

BY FRANCIS BELLO

A panel of distinguished industrialists and scientists here talk over the goods and evils of an impending industrial revolution, brought on by automatization of factory practices. If you want to know the future of American productivity, nothing is more revealing than the shoptalk of the men who ultimately determine it.

HISTORIANS will have to decide if a Second Industrial Revolution actually got under way sometime around the middle of the twentieth century. By popular definition the second revolution will replace man's sensory apparatus and brain—in doing routine jobs—as the first replaced human (and animal) muscle power. The first revolution, still continuing, *mechanized* manufacturing processes. The second will *automatize* them; i.e., it will remove man from the manufacturing operation itself and relegate him to maintenance and supervisory roles. According to this definition, automatization has made significant progress in the oil, chemical, and similar fluid-

handling industries, but relatively little progress in industries that process metal.

To learn how automatization is progressing in the metalworking and assembly-line industries, *Fortune* invited representatives from ten companies, each with a large stake in the Automatic Factory, to participate in a round-table discussion. Also present: an authority on servomechanisms from M.I.T., an eminent sociologist from the University of Chicago, and E. W. Leaver and J. J. Brown, the authors of *Fortune*'s November, 1946, article, "Machines Without Men." Participants were sent a reprint of the 1946 article, which served as a take-off point for the discussion.

The Round Table seemed to agree that the next eight to ten years—because of a tight labor supply resulting from the low-birth-rate Thirties—will provide industry with an ideal opportunity to go all out on automatization. One estimate presented to the Round Table was that industrial productivity might need to be over 40 per cent higher in 1960 than in 1950 if the presently surging population is to enjoy a standard of living moving upward at the rate to which the U.S. is accustomed. Since manufacturing productivity in the past has risen only about 3 per cent per year, it is evident that the nation's engineers will have to exploit the potentialities of automatic production without stint.

At the same time the day-long meeting made it clear that U.S. industry is so complex, and progress in specialized areas so rapid, that the men charged with increasing the nation's productivity have great difficulty exploiting modern technology to full advantage. With *Fortune* Editor Eric Hodgins as moderator, the Round Table concentrated on seven questions. The first:

1. *Is Automatization Gaining Momentum?*

The Round Table agreed that incentives to automatize are compelling and that new technological developments provide engineers with expanded means for doing the job. Much of the documenta-

tion supporting this conclusion was to be found in the brief opening remarks made by each Round Table member.

MR. BEARDSLEE (*General Electric; manager, manufacturing engineering services*): We have been working on this problem of mechanization for some thirty-five years.* A formal organization in each operating department is assigned specifically to the job of mechanizing our operations. Within the last two years we have divided these organizations into two sections: those fighting day-to-day fires, and those that are looking down the road, and each group does nothing but that.

I think that the forces within our company that are really motivating us to work aggressively toward this end of an Automatic Factory are probably, first of all, the president, who is insistent that we make progress in this field; second, the willingness of our management to invest heavily in this approach, which we don't think is experimentation. We think it is coming. We think it will probably be an evolution, not a revolution. We have had enough failures and enough successes to have a pretty good feel of the subject.

MR. EMMERT (*General Motors; executive in charge, facilities and processes staff*): Those of us who were actively in management of our factories fifteen or even ten years ago remember that we had great difficulty in making changes in our methods, particularly if they resulted in fewer people for that particular operation. Now just a few years ago, when Mr. Wilson was working on a five-year contract with the union, he came up with the idea that our employees are entitled to share the benefits of those improvements. And out of that came the thought of the annual improvement factor, which, in our case, was 4 cents per hour for each year of the five-year contract.

It is pretty clear that much of our progress is brought about by competition. Most of our mathematics are quite simple. We pay

* *General Electric uses the term mechanization to include what others would call automatization.*

so much for material, so much for labor; we add that up and subtract it from what we sell it for, and we hope the figure is plus; and if it isn't we have to work on it.

MR. PATTERSON (*Ford; general manager, engine and foundry division*): I am a veritable neophyte at the sort of Automatic Factories these scientific fellows write about. What they are talking about is in dreamland.

The automation we have installed enables us to get the capacity from the machines which were formerly controlled by manual handling. Automation eliminates much of the drudgery. It makes for safer operation, and also improves our quality. And, of course, we are also very anxious to produce at least cost.

Mr. Tasker of Republic Aviation and Professor Pease of M.I.T. explained that their interests in automatism were rather special and, as it happened, closely related.

MR. TASKER (*works manager*): Our current interest in automatism is forced upon us by the government sponsorship of the heavy-forging-press program. M.I.T. is trying to help us solve the problem of machining forgings to finished size automatically in the least possible time.

We have just finished what, in our industry, is considered a very high, long-term production schedule. We started in 1946 and have just concluded building one type of airplane, the F-84. We have built between four and five thousand units. Throughout the history of this airplane, during production, we have accepted an average of 315 engineering changes per week. Gentlemen, the aviation industry is still a custom operation. We hope to achieve automatism in the near future. [Laughter.]

PROFESSOR PEASE (*director, servomechanisms laboratory*): I think you have all heard of M.I.T.'s program-controlled milling machine. As Mr. Tasker says, we are trying to apply the machine to the problem of finishing forgings that come off the big presses. The forgings

can be quite intricate and the problem is to code onto a punched tape all the instructions necessary for the milling machine to produce a finished forging to specification, without human intervention.

The remaining industry members of the Round Table represented companies interested primarily in selling the components of the Automatic Factory to others. There were seven of these companies: Cross Co., Electronic Associates (the Canadian firm of E. W. Leaver), International Business Machines, Minneapolis-Honeywell, Pratt & Whitney, Raytheon, and Remington Rand. To this group must be added General Electric, which sent Dr. Rader to represent its specialty control department.

MR. CROSS (*executive vice president*): My company builds transfer and other types of automatic machines, and our interest in the Automatic Factory is a purely selfish one. We are interested in promoting it for our own good.

MR. LEAVER (*co-author of the 1946 article*): Having been mixed up with radar and electronics for a considerable period of time, I am interested in the possibility of applying electronic concepts to industrial problems. My company and I think automatism has electronics in its future.

DR. HURD (*I.B.M.; director, applied-science division*): We are interested in the field of computation, and in all discussions of automatic controls, computers enter. We are interested in the collateral problems of inventory control, production scheduling, etc., which are problems more directly not in the field of automatic production, but of an Automatic Factory—indeed, of an automatic industry.

MR. SCHUCK (*Minneapolis Honeywell; chief, aero research*): Honeywell's interest in life is automatic control. That interest is as broad as the field of automatic control is broad. We feel that our experience along these lines typifies the basic and inexorable trend of man's economy, and we believe our future lies in furthering that trend.

MR. JAEGER (*Pratt & Whitney division of Niles-Bement-Pond; assistant manager, machinery engineering department*): We, like Mr. Cross, are extraordinarily selfish about this business of automatism and machinery which is automatized. We are selling it. We also have a gauge end to our business. If this Automatic Factory is to appear, not only must the work be produced and gauged, but gauging and sensing information has to be fed back to the input of a machine, so that it can repeatedly bring its tools to the center of tolerance, so that the machine will not drift. It is this closing of the cycle that has still to be achieved, and we are in the middle of it.

MR. NICHOLS (*Raytheon; manager, research division*): We are trying to develop apparatus for the automatic control of more or less existing machine tools and handling machinery For instance, the addition of contour controls for milling and turning, to permit the following of templates, or perhaps, at some future time, to follow data coming in on punched cards, tape, or whatever. We also have fairly large activities in the modern computer field.

MR. SEARES (*Remington Rand; vice president and director*): The top management of Remington Rand decided nearly ten years ago to see how electronic devices might be applied either to the office or to the factory. To this end, we initiated research and development projects at our Norwalk Advanced Engineering Laboratory, and we have also purchased two companies: the Eckert-Mauchly company, developer of the Univac, and Engineering Research Associates. These three sizable operations have made it possible for us to advance greatly all phases of automatism.

DR. RADER (*G.E.; general manager, specialty control department*): G.E. got into the control business to satisfy the needs of industry. I would say that the greatest motivation we have is the customer who says, "I have a problem, help me solve it." In general, the great majority of our problems still deal with individual functions, and of course we are very much interested in tying things together in ways that will make good economic sense.

While the foregoing remarks indicated strongly that automatism might be gaining momentum, the Round Table was asked specifically whether it believed this to be true, or if, conceivably, there might be an actual slowdown because the easier advances had already been made.

DR. RADER: Nothing has been said today about the new materials, the new developments, which have been coming along at quite some speed recently, which are going to make automatism a lot easier to accomplish. I have reference to things like pressure-sensitive barium titanate and light-sensitive cadmium sulfide, plus all the semiconductors, transistors, and so on, which we need to make a control system work.

One thing I want to mention especially is the magnetic amplifier, which eliminates a lot of objections to electronics. It is not fragile. It represents something really new in electric circuitry.

MR. SCHUCK: My point is along this line: the requirements are continually getting more complex. The demand for higher and higher performance forces us to greater automatism just to make it possible for the human being to keep hold of the process.

Despite many declarations that industry was pushing automatism with all its energy, a few Round Table members thought the job should be moving ahead faster. For example:

MR. LEAVER: I realize only too well that our economy and our production facilities are very large, very complex operations, and certainly you cannot revolutionize them overnight. However, I don't believe that a completely evolutionary development necessarily represents the best approach.

Where industry has attempted to automatize, Brown and I feel that it has too often got off on the wrong foot by trying to create highly specialized machines. I don't think you can escape the conclusion that some of these machines rival Rube Goldberg. They have little chains and gears and rods popping in and out all over, and it seems as though the thing may have gone slightly astray. One

of the reasons we are told that automatism is not applied more than it is lies in the great cost involved, and you can see that a great many of these special-purpose machines have to be custom-made, and of course they are costly.*

> It is the Leaver and Brown thesis, first presented in 1946, that automatism need not be prohibitively costly if one keeps basic operations rather than the product in view. They argued that it should be possible to link basic units together to perform any desired sequence of operations.
>
> Professor Riesman wondered how receptive industry would be to the Leaver-Brown thesis or to any other unconventional proposal.

PROF. RIESMAN (*professor of social sciences, University of Chicago*): I have somehow the feeling that when you go to your management for permission, for budget, for manpower, you may meet certain resistances based on a conventional concept of the factory as a place with people in it, but concealed under the surface is, "This is visionary."

DR. RADER: I don't think you have got the concept of the modern factory at all, Professor. Actually, there are many, many men at work all the time continuously analyzing all the processes we have. They are the ones that come up with ideas for doing things more efficiently.

PROF. RIESMAN: I have a sense of that kind of activity and energy. I was thinking when Mr. Patterson was talking about his new Cleveland engine plant, suppose someone had gone to Ford man-

agement and had said, "Before you build this huge plant which puts together all these piecemeal developments, and organizes many of them brilliantly, let us try the Leaver-Brown concept of process orientation rather than product orientation." Where do you think he would have gotten?

MR. BEARDSLEE: We could tell you. . . . Right outside the gate.

2. *What Are the Most Difficult Jobs to Automatize?*

While Professor Riesman's hypothesis received no support from the Round Table generally, the engineers spent much time talking over obstacles that blocked the way to greater automatism. Round Table members agreed with Mr. Emmert that automatism is most easily applied "to any series of operations which can be sequentially determined ahead of time." Even this, however, may not be enough. Take, for example, the problem of automatizing the assembly line:

MR. PATTERSON: From my humble point of view, trying to automatize an assembly line is a terrific problem. Automation of subassemblies will have to be the first step. We have the greatest difficulty in getting machine-tool builders to give us subassembly machines with any degree of productivity. As for the final automobile assembly line, I think it would be quite a task to assemble the cloth trim mechanically. We will solve it someday, but it won't be tomorrow.

MR. EMMERT: You can easily imagine the complication of assembling as compared with machining. However, we are making considerable progress in automatizing subassemblies. For instance, we have a radiator-cap assembly machine. It puts together some stampings, rivets, screws, gaskets, and springs, and inspects the finished assembly.

Fortune asked if a "hand-laying" machine for assembling automobiles could not easily be built if someone were willing to spend the effort that M.I.T.'s wartime Radiation Laboratory put into the development of gun-laying radar.

PROF. PEASE: Well, $100 million buys a lot of effort, but I think it ought to be made clear that the basic operation of gun laying is infinitely more simple than the basic operations of assembling even a radiator cap. You have only two motions to deal with in gun laying. The problem is to do it accurately and fast. Assembly is a manipulation problem with more dimensions and many more variables.

MR. PATTERSON: Mr. Cross, would you be willing to give me a fixed price to build a machine for assembling engines?

MR. CROSS: Of course, we probably could dream up a price, and you would probably reject it.

MR. PATTERSON: That is exactly the answer I wanted.

MR. CROSS: I doubt very much that we will ever see an automobile engine assembled automatically in my lifetime.

MR. LEAVER: If one makes up one's mind that a problem is insoluble in the beginning, it always is. I don't think we should expect to see a machine that will put together the present Ford engine. After all, it is designed to be put together by men. You should not be astounded that it will not go together easily automatically. Surely machine-design and product-design people can arrange it so the goods go together easier, or better, or that registry is easier than it is now.

DR. BROWN (*co-author of "Machines without Men"*): We've got a lot of men on these assembly lines. Now men, by definition, are difficult and tricky things to play around with. You have employee-relations men, time-study men; you have training and education directors; you have personnel men, washroom men, cafeteria men. You have got a public-relations problem. That all costs money. My point is this: that if we could take some of the money that we are spending in trying to ease the pain of our assembly-line personnel, and apply that money for some research to get the men out of there entirely, we would be far better off in the long run.

MR. BEARDSLEE: We have been taking quite a few socks at Mr.

Leaver and Dr. Brown today; so I should like to get on the other side of the fence and say in their defense that, with a little vision and doing some of the things they cited, we have made considerable progress in mechanizing production of one of our electrical products. We saw that the wires in the chassis of this product presented an almost impossible mechanization problem. It was comparable to putting the cloth trim in an automobile.

Our approach was to redesign the chassis so we could substitute a stamped circuit for the individual wires. To mechanize the assembly of the other circuit components we fixed it so that all components could be fed automatically from the top, so that we don't need any turnover mechanisms. We have asked manufacturers to supply resistors and other components in magazines ready to be fed into our assembly machine. The components are joined to the stamped circuit automatically by dip-soldering.

I would say that probably on assembly more will be done by product engineers than by machine-tool builders.

The last word on the problem of the assembly line came from a somewhat unexpected source, the Round Table's mathematician.

DR. HURD: To me the most provocative problem of all is how do you assemble an assembly line. It seems to me than the most useful analogy which I can see for the assembly line is that the assembly line—or more generally, a complete production line—is like a computing machine.

Now this is a useful idea, because a modern computing machine is a completely automatic thing; but it is completely automatic in a very general and flexible sense.

What you do is that you specify a certain number of operations, and then you also specify a form of control which can provide interconnection between these basic operations. Thus it is that with about thirty-two operations like add, subtract, multiply, divide, compare, transfer, and so forth—you can solve any computational

problem in the world, provided that you translate the problem into those basic operations.

The provocative aspect of this analogy is that if you can organize a production line logically from this point of view by defining these basic elements—then you can evaluate the economics of the interconnections between the various elements and find out whether this idea can be advanced with a great amount of acceleration.

Rather surprisingly, no one, at the time, seemed to grasp the striking implications of Dr. Hurd's remarks. Perhaps without realizing it himself, Dr. Hurd had restated precisely the three fundamental concepts that Leaver and Brown hammered at seven years earlier: process, flexibility, and electronic linkage. By drawing his analogy from the electronic computer, which exists, Dr. Hurd helped to make clear that the Leaver-Brown argument may not be so visionary after all. The analogy is somewhat ironic, however, because Leaver and Brown are quite insistent that the giant computer itself has no place in the factory they visualize—a point that comes in for discussion later. From Dr. Hurd's point of view the analogy is ironic because it envisions a factory *like* a computer, and does not necessarily require that the factory be controlled by one.

3. *Why Have the Oil and Chemical Industries Become So Highly Automatized?*

The Round Table members most familiar with oil and chemicals were Mr. Schuck, whose company, Honeywell, is one of the leading [illegible] and Mr. Nichols, who

question No. 3.

MR. NICHOLS: A man with his hands can easily handle solid objects. It is pretty darned difficult to make a machine that can do what the ten fingers—along with the eyes—can do. Well now, you go over into the processing industries, which tend to involve the processing of liquids or gases, and man is awfully poor at trying to handle liquids and gases.

No one ever thinks of arguing whether or not to build a mechanical device, namely a pump, to transmit liquids from one place to another. Then you go further and you measure volumes and arrange to change the output of the pump automatically.

MR. SCHUCK: Wouldn't you say that inherent instability of gas and liquid flow has had a lot to do with forcing the process industries to automatic control and what we call continuous feedback? It is fairly easy to make a relatively stable machine that will stamp or grind out parts to a given dimension.

MR. NICHOLS: I think that is right. You can build an automatic screw machine which has, as far as I am concerned, no feedback at all, and yet it can turn out screws satisfactorily for an eight-hour shift.

You can't do this in a chemical plant because, in the first place, the ingredients coming in will not be that carefully controlled.

> This seemed to answer a question that was raised by *Fortune:* "Would the metalworking industries need as much feedback control as now exists in a modern refinery to achieve the same degree of automatism?" Messrs. Schuck and Nichols indicate the answer is no. *
>
> *Fortune* next asked if modern oil and chemical plants were really as automatic as the average person had been led to believe.

MR. NICHOLS: I think the chemical and petroleum plants are nowhere near yet at the peak of being made completely automatic. They contain a tremendous number of regulators and controllers, but the important feedback element is still carried out by a man. If, for example, the specific gravity of the product begins to go off specifications, you don't automatically, without human intervention, go back and change things. You have an operator who decides how to bring this gravity back.

One of the big jobs yet to be done in oil and chemical plants is to

* *Dr. Brown: "To achieve what I call automatism (not just what Ford calls 'automation') the metalworking industries would require much more control equipment and feedback than exist in refineries today."*

be able to sense the actual variables that they really want to control, and to be able to hook those into perhaps fairly complicated scheduling or computing mechanisms to replace the judgments now made by men.

4. *Is Electronics the Key to the Automatic Factory?*

To nontechnical people it seems fairly obvious that if electronics is the key to inconceivably swift and clever military devices it ought to be a vital key to the Automatic Factory. The Round Table split as sharply on this point as on any other.

MR. LEAVER: Sensory devices and control devices of various types are frequently most easily produced if one uses electronic principles. Electronics could tie together various aspects of machines to make them self-controlled—automatic—so it seems to me that electronics is an essential key or common denominator.

MR. CROSS: In our machines we have all the basic elements of control that Mr. Leaver and Dr. Brown speak of, but we don't use electronics because generally it is too delicate for the machine-tool industry. We do it in what Mr. Leaver calls a Rube Goldberg way, because it has to be rugged.*

I will take issue with some of the people here who have interests in promoting program devices. The difficulty with such devices as the punched card or magnetic tape is that they have no flexibility. Programs have to be changed rapidly in production, and even the time it takes to punch a tape would be too long for us.

We now program the changing of tools. If we were to use a punched-card system to program our tool changes, we would have to create a new card-punching department to take care of the program problems.

MR. LEAVER: How do you change programs now?

MR. CROSS: Manually.

* *Mr. Leaver: "I thought this statement hardly needed refuting, electronic devices are used by the hundreds of thousands in factories."*

MR. LEAVER: Every few minutes?

MR. CROSS: No, whenever trouble occurs.

DR. BROWN: You call in your setup man, in other words.

MR. CROSS: No. I won't take the time to describe what we do.

MR. LEAVER: Would it not be practical to combine with the program a little bit of sensing information and collate the two?

MR. CROSS: We have a lot of sensing information. We need certain things more

MR. LEAVER: May I ask one question? Is the reason that you don't collate your sensing information with your program information because it would be difficult to do in view of the form in which your program is, on the one hand, and the information about what is happening, on the other?

MR. CROSS: You said a mouthful there, but I can say yes to it, I guess.

DR. HURD: You'd better not!

> Dr. Hurd meant, of course, that Mr. Leaver had set an electronic trap for Mr. Cross. Evidently Mr. Cross had no intention of avoiding it, feeling as he does that electronic devices are too delicate for the factory floor.

MR. CROSS: If we could find some way of doing mechanically what the man does now, that is, looking, listening, feeling—to detect that a tool is worn out—and if we could feed that information back to our control apparatus, we would be able to take a big step forward. If any of you have any electronic devices that will do such things, we would like to see them very much.

> At the break for lunch, Dr. Rader lost no time reaching Mr. Cross to tell him that G.E. had electronic devices that might meet his requirements.

5. *Should the Automatic Factory Be Flexible?*

In their 1946 article, Leaver and Brown argued that the Automatic Factory should and could be extremely flexible.

MR. CROSS: Now the main difficulty with the original Leaver and Brown article, as I see it, is the fact that they fail to realize that we have to deal with physical objects. Certain elements of the processing equipment have to conform to the shape of the product. But we do not make any more of it conform than is absolutely necessary; we strive for flexibility.

Our transfer machines are made up like an Erector set. We have certain basic components—electric motors, drilling heads, etc.—and we can reconstruct them so they will machine parts of a different size and shape. Maybe 60 or 75 per cent of a transfer machine will be made up of these basic components, which are, in a sense, what Leaver and Brown talk about—operation machines.

We have even gone a little beyond that. We have devised a system of mounting the piece of work on a pallet, which is moved through our machines, from station to station. We can even machine two or three different palletized parts on the same machine.

DR. HURD: When Mr. Cross speaks of Erector sets and tells us that his machine is made of basic components which can be rearranged to make a variety of parts, it makes me think that his concept and the concept of Leaver and Brown are much closer together than I had imagined earlier.*

MR. PATTERSON: There is much flexibility in our concept of automation, but—with all due respect to Mr. Cross—we don't want any misunderstanding. If we are machining engine blocks, we can't

move a few levers and start machining cylinder heads. There are no machines like that that I have seen anyhow.*

6. *What Role for the High-Speed Computer?*

Probably no aspect of the Automatic Factory has aroused so much vague and confused thinking as the role to be played by the modern high-speed electronic computer. Inasmuch as such machines can compute infinitely faster than a man and perform prodigious feats of logical reasoning, it seemed obvious that they ought to fit into the Automatic Factory somewhere. But where?

DR. RADER: Actually, the question seems to be one largely of economics. It is hard to get a man to buy a computer and a control for his machine that might cost five times what the machine costs. However, it may be possible to transfer information from a computer onto a tape, which, in turn, can run a machine. In this way one computer can service a large number of machines. That is one of the possibilities G.E. is investigating.

MR. LEAVER: I don't myself see a computer set up to operate a factory at the moment. I think myself that Automatic Factories will grow out of small individual automata, which later can be integrated to operate together.†

In reference to the suggestion heard in some quarters that an Automatic Factory might simulate the functions of a living organism, all under control of a big brain, I would like to point out that

* Dr. Brown. "*Flexibility is the most vexatious problem in the whole field of* [illegible] *the manufacturer* [illegible]

[illegible] *Electronics, when he questions the role of the computer.*"

most organisms are not necessarily brain-controlled. Most of them are reflex-controlled, and I think a good many factories will end up being the same way.

As this discussion developed it was evident that many Round Table members wondered if the representatives of either I.B.M. or Remington Rand would be able to present a good case for the electronic computer. At this point it should be noted that the Round Table had viewed a short film, loaned by Ford Motor Co., called *Technique for Tomorrow*, which was a guided tour of Ford's new Cleveland foundry and engine plant. In his closely reasoned discussion of the role of the computer in the Automatic Factory, Dr. Hurd of I.B.M. referred to what he had just seen in the film.

DR. HURD: I am going to present an argument which indicates that when a sufficient degree of automation is attained, we in fact need a very high-speed and a very large-capacity automatic digital computer.

I will begin by referring to what was illustrated in the Ford movie. We saw there the marvelous steps taken forward in the last few years from the standpoint of automation. We also heard such words from the narrator as "This requires skill and experience," "This requires just the right touch."

It seems that what we have got to get at is replacing these human judgments, and in replacing human judgments, perhaps the crucial question, or the crucial distinction between the human as the judger and the machine as the judger, is that the human is a *multi-channel* device. This has not been explicitly stated here, but it is extremely important in the economics of the situation. The human is a multi-channel communicator, multi-channel device. He feels, touches, smells, and sees, and instantaneously takes all these factors into his brain, arrives at a rationalization, and makes a decision.

However, most of the *digital* computers are still single-channel devices. They operate with extreme speed, but perform exactly one operation at a time. The machine is immediately at a disadvantage

with the human who is a multi-channel communicator and a multichannel communicator analyst.

I think then that you can perceive that the machine will need to go through a tremendous number of operations in a comparatively short time, because it is only a single-channel communicator. Therefore it must be very fast. And if it is fast, it has got to be large. This means an expensive machine. You cannot stand this economically; so you think of putting all of this kind of decision-making for the whole production line into one machine.

So I have advanced an argument here, and please notice that it is only an argument because nobody has ever tried this.*

Dr. Hurd made it clear that all this was for the future. How far into the future could be judged from an observation he made at another point.

DR. HURD: The problem of the Automatic Factory, if couched in sufficiently general terms, brings you immediately to the fact that there is not a sufficient amount of information concerning the appropriate mathematical models of the Automatic Factory. Second, you will find that you can prove that there are no mathematical methods known of sufficient strength to solve some of the problems in the mathematical formulation of the Automatic Factory. We need better mathematics.†

7. *Is the Automatic Factory Socially Desirable?*

For its last question of the day *Fortune* asked: "How much weight should industry give to the social desirability of the Automatic Factory?" Almost without exception Round Table members feared that most workers would view the emergence of the Automatic Factory as a threat to their security.

* Mr. Leaver: *"Dr. Hurd apologizes for the limitations of computers, but only he would think of using a computer at all. There are a great many simple electronic devices which can bypass the need for a digital computer completely."*

† Mr. Leaver: *"This is a nice general statement, but relative to the problem at hand the mathematics are required only if you use a piece of apparatus [i.e. a computer] which is really unnecessary to the job at hand."*

The comments of Mr. Hitchings, manager of Ford's economic-analysis department, were typical.

MR. HITCHINGS: I think a better selling job has to be done on the social desirability of increased mechanization, because even though many people accept the idea that well, yes, it is a good thing to have increased productivity, they always point to the particular situation and say, "Well, that is bad, because you displace labor in that particular spot."

MR. JAEGER: I don't think we are consciously trying to ease the burden of our workers, nor consciously trying to improve their standard of living. These things take care of themselves. They have a feedback of their own that closes the loop automatically. I don't think that it is the part, nor can it be the part, of industry to try to plan the social aspects of this thing.

Professor Riesman, the Round Table's sociologist, surprised some of his listeners by supporting Mr. Jaeger.

PROF. RIESMAN: I have a feeling that it is a kind of grandiosity for each trade to worry about the social consequences of its work. Journalists are inclined to be awfully pious about how effective they are. Social scientists worry about manipulation before they have any cause to worry, because they aren't that good. Engineers worry about the consequences of their achievements.

I think of the Rust brothers who used to worry about their cotton picker in the early 1930's. It turned out that some of the people displaced by it are working in the Boeing plant in Wichita. It was senseless to worry. It wasn't their job.

He didn't mean, however, that sociologists might not be entitled to worry about the consequences of the Automatic Factory.

PROF. RIESMAN: I am interested in leisure, and the feeling of many Americans that leisure is a threat, a problem, burden, or hazard, rather than as man has thought for centuries, a clear gain—and I

have somehow the feeling that this fear of leisure may be one of the factors holding back further automatization.

My second interest is in work itself as an emotional hazard for men and women which can be relieved by automatization. I think, for instance, of a study by William Whyte (not *Fortune's*) of the crying waitress—why waitresses cry, caught as they are between irascible customers and even more irascible cooks.

When I see an Automat, I would like to bow down and salaam to it as a blessing, because it gets rid of the crying waitress, only qualified by my feeling that there may be some waitresses with a vested interest in their miseries, and I think the problem of the Automatic Factory and transition to it is that of taking care of those with a vested interest in their deformities, such as a masochistic need to work too hard.

I am also thinking of the one institution in our society where automatism has moved very fast, namely the home, and where we have a whole cadre of unemployed people, namely children. We are bringing up, it seems to me, a generation of people who lack the industriousness and work-mindedness of the present generation. The development of the Automatic Factory is salutory if only to keep the level of production rising to meet the growing population's growing needs.

Of the technically trained people at the Round Table, only Dr. Brown and Mr. Leaver seemed deeply concerned about social consequences.

DR. BROWN: My view about the whole field of automatism is that it is a subject whose far-reaching implications almost no one has grasped. Correctly applied, it will cause a change in Western living habits and attitudes toward work that will dwarf the effects of the Industrial Revolution. My hope is that because of what I think is our greater awareness of human values, we can find a way to reap the vast benefits of automatism without incurring too many of its serious penalties.

Editorial afterthoughts

The Round Table offered further proof, if any were needed, that technology continues to be the most explosive force in modern society. It was also quite obvious that a company like General Electric, whose interests range from transistors to nuclear reactors and gun-laying radar systems, is in a better position than most to pull together the diverse components needed to automatize effectively. When this breadth of interest is combined with a determination to make progress, as Mr. Beardslee indicated, great things may be expected.

In the nature of things, man will create an Automatic Factory—as he climbs Mount Everest, and aspires to reach the moon—for reasons no one has ever clearly expressed. Except that he is man.

October, 1953

15

The Information Theory

BY FRANCIS BELLO

Man's patterns of communication are widely varied. But they are also often plain cumbersome. Under the radically new information theory, communications researchers, psychologists, linguists—all who deal in the realm of meaningful contact between human beings—are charting new ground, thanks to what may turn out to be the most important new scientific theory of our generation, as relativity was of the last.

GREAT SCIENTIFIC THEORIES, like great symphonies and great novels, are among man's proudest—and rarest—creations. What sets the scientific theory apart from and, in a sense, above the other creations is that it may profoundly and rapidly alter man's view of his world.

In this century, man's views, not to say his life, have already been deeply altered by such scientific insights as relativity theory and quantum theory. Within the last few years a new theory has

appeared that seems to bear some of the same hallmarks of greatness. The new theory, still almost unknown to the general public, goes under either of two names: communication theory or information theory. Whether or not it will ultimately rank with the enduring great is a question now being resolved in a score of major laboratories here and abroad.

The central teachings of the theory are directed at electrical engineers. It gives them, for the first time, a comprehensive understanding of their trade. It tells them how to measure the commodity they are called upon to transmit—the commodity called "information"—and how to measure the efficiency of their machinery for transmitting it. Thus the theory applies directly to telegraph, telephone, radio, television, and radar systems; to electronic computers and to automatic controls for factories as well as for weapons.

It may be no exaggeration to say that man's progress in peace, and security in war, depends more on fruitful applications of information theory than on physical demonstrations, either in bombs or in power plants, that Einstein's famous equation works. As might be expected, military applications are coming first. For example: The "Distant Early Warning Line" of automatic radar stations, stretching from Alaska to Greenland, almost certainly incorporates more of the lessons of information theory than does any other communication system yet devised. The warning line was designed by the two organizations that should know more about the theory than anyone else: Massachusetts Institute of Technology (working through its Lincoln Laboratory) and Bell Telephone Laboratories.

The theory has an unusual joint origin. To M.I.T.'s eminent mathematician, Norbert Wiener, goes the major credit for discovering the new continent and grasping its dimensions; to Claude Shannon of Bell Laboratories goes the credit for mapping the new territory in detail and charting some breath-taking peaks. Wiener's basic contribution was to recognize that communication of infor-

mation is a problem in statistics, a view he first stated clearly in a secret World War II document that dealt with the problem of shooting down airplanes. He followed this in 1948 with his now famous book *Cybernetics, or Control and Communication in the Animal and the Machine.* The same year Shannon published his great work, *A Mathematical Theory of Communication,* aimed specifically at the electrical engineer.

The fascination of the theory, as *Cybernetics* indicates, is that it insists on thrusting beyond the confines of electrical engineering. In particular it is Wiener's belief, shared by many others, that one of the lessons of cybernetics is "that any organism is held together by the possession of means for the acquisition, use, retention, and transmission of information." Naturally, therefore, attempts are being made to use information theory in a dozen fields from psychiatry to sociology. In a few fields, notably psychology, neurophysiology, and linguistics, the theory has already been applied with considerable success.

What information means

What the theory does for the first time is provide a precise unit of measure for the "amount of information" in various broad classes of messages. The class may be represented by a voice on the telephone, a picture on a television screen, the language of Shakespeare, or the music of Beethoven. When the engineer has used the theory to measure the information content, or information density, of such messages, he can tell how large his transmission channel must be to carry each.

Information, as used in the theory, is very carefully defined, and information theorists have trouble forcing people to stick with the definition. To Wiener and Shannon, information is contained, to great or less degree, in any message a communication engineer is asked to transmit. He is not interested in semantics or meaning; he must assume that even gibberish may have meaning

The Encoding of Information

To transmit information—words, music, pictures—the communication engineer must encode it. A central teaching of the Wiener-Shannon Theory of Communication is that encoding should take advantage of the statistical nature of messages.

One code system of great value uses only two symbols, 0 and 1, which are called binary digits, or "bits" for short. The diagrams on the following page show how ordinary letters can be coded into bits. If two letters, A and B, appear in a message at random (hence with equal probability), an efficient code will let A = 0 and B = 1 (or vice versa). Similarly, each letter in a four-letter "alphabet," A, B, C, D, will require a two-bit code, *provided*, again, that one letter is as probable as another.

Suppose, however, an information source has an A-B-C-D alphabet but uses some letters more frequently than others. Then, says the theory, each letter does not carry a full two bits' worth of information, hence does not deserve a two-bit code. If a source creates messages that contain, on the average, half A's, one-quarter B's, and one-eighth C's and D's, it can be shown that an efficient code lets A = 0, B = 10 ("one-zero," not "ten"), C = 110, and D = 111. With this code a typical message BDAAABCA becomes: 10111000101100. So coded the total message contains only fourteen bits, or 1¾ bits per letter—not two bits.

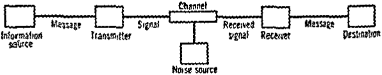
The basic method for creating an efficient code is shown at the bottom of the next page. The symbols are hung on an asymmetrical "mobile" so that the first decision point (marked 0 or 1) divides the symbols into two equally probable groups. (Here, A is as probable as B, C, and D combined.) The next decision point again divides the remaining symbols into two equally probable groups, and so on. The "mobile" is rigged correctly if random trips through it will generate messages that have the same letter frequencies as those composed by the information source itself.

Information theorists say that it is possible to make a similar,

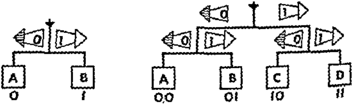
Continued on page 275

The Encoding of Information (CONT.)

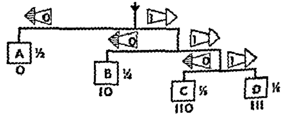
Communication's Basic Network



Communication theory deals with the generalized communication system shown above. The key to efficient communication is maximum compression (i.e., proper encoding) of the message at the transmitter.

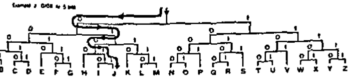


The symmetrical "code mobiles," above, indicate the basic method for establishing an efficient binary-digit (0 and 1) code for two or four letters, *provided the letters appear in a message with equal frequency.* The code is determined by the "0" and "1" signposts that are passed en route to each letter. The asymmetrical "mobile," below, yields an efficient code if the engineer is trying to transmit messages composed half of A's, one-quarter of B's, and one-eighth of C's and D's.

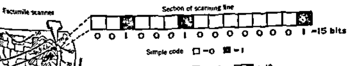


but gigantic, "code mobile" that would provide a coding of maximum efficiency for ordinary literary English. As the diagram indicates, an ordinary (equal-probability) mobile for the alphabet calls for between four and five bits per letter (4.7, to be exact). Shannon's surprising conclusion is that sensible written English carries, on the average, only about one bit of information per letter. (For details, see text.)

The alphabet in 4.7 bits per letter



The weather map, bit by bit



To transmit a weather map, facsimile uses, in effect, a binary code. As the scanner traverses the map it sends a pulse, or 1, at each unit of black space, and "no pulse" or 0, at each unit of white. The trouble with this simple system is that it spends so much time sending nothing (i.e., white space). Recently the Microwave Research Institute (Polytechnic Institute of Brooklyn) demonstrated a more efficient system for transmitting maps. The system uses a code similar to the "better code" shown. The object is to transmit long stretches of white space quickly, i.e., in short code, at the penalty of sending infrequent black units in a code longer than the present simple code.

tensity of every dot on every one of the 525 scanning lines 30 times a second. To do this required a bandwidth of some 4 million cycles, or nearly 1,000 times that required for ordinary radio. Shannon recalls that one of the questions motivating his early work was: could television be compressed into a smaller bandwidth, or couldn't it?

While information theory now shows that it can, no one has progressed much beyond paper plans, for the job is extremely tricky and takes a lot of hardware.

It is precisely here that the value and power of a good theory become difficult to describe. A theory builds no machinery. But inevitably, when good theories are enunciated, they make machinery easier to build. "Before we had the theory a lot of us were deeply troubled," says Jerome Wiesner, director of M.I.T.'s Research Laboratory of Electronics. "We had been dealing with a commodity that we could never see or really define. We were in the situation petroleum engineers would be in if they didn't have a measuring unit like the gallon. We had intuitive feelings about these matters, but we didn't have a clear understanding."

One "bit" of information

To provide a measure for information, which makes it possible to measure the "something" in different sorts of messages, information theory builds from the simplest of all bases. It considers two symbols, say A and B, and the way they may be combined into messages. We have already seen that an endless string of A's presents nothing that needs transmitting. Information begins with uncertainty—with the first B. As more B's are mixed in with the A's the engineer has to send out more signals. In the extreme case when there are as many B's as A's and they appear at random, i.e., unpredictably, the flow of signals—hence the flow of information —reaches a maximum. The simplest possible code for A and B

is o and 1. If the engineer sends the 1 over a channel as one electric impulse, and the o as "no impulse," he has achieved all the economy possible.

Thus the engineer is working hardest, in the simplest case, when transmitting two symbols of equal probability. This suggested to Wiener and Shannon that *the unit of information be defined as that which makes a decision between two equally probable events.* This unit was baptized the "bit" because the symbols o and 1 are technically known as "binary digits," which someone had previously abbreviated to "bits." Thus to transmit a random string of A's and B's the engineer has to transmit one full bit of information, either o or 1, every time the message source utters one letter or the other.

By stringing together code groups composed of bits—just as Morse used dots and dashes—it is possible to code the entire alphabet of twenty-six letters, or "alphabets" of any desired length. A code group two bits long provides four combinations, oo, o1, 1o, 11, hence can be used to encode a four-letter "alphabet," say, A, B, C, D. A three-bit code can be arranged in eight possible combinations: ooo, oo1, o1o, o11, 1oo, 1o1, 11o, 111, hence will specify an eight-letter alphabet. Note that as the code lengthens by *one* bit, the number of combinations *doubles*. Thus an eight-bit code provides 256 combinations starting with oooooooo and ending with 11111111.

Information theory tells the engineer that his codes are efficient only when each o and 1, i.e., each bit, is working just as hard as it can. When this is achieved, says the theory, the engineer can count up the number of bits he has used, and this will tell him the *net* amount of information in the original message.

Since the engineer, obviously, cannot be expected to sit down and test every possible way of encoding a message into o's and 1's, the theory provides him with an equation that gives, in bits, the amount

of information per symbol in any message—be it speech, music, or pictures.* All the engineer has to put into the equation is the relative frequency with which each symbol appears in the message. This is not hard to do for a single message, but the answer obtained in this way is not very useful. What should go into the equation are frequencies with which *groups of symbols* are used in a large sample of messages.

This concept is easiest to follow if we consider written English. Morse dealt only with frequencies for each letter. However, he might have counted the frequencies of letter *pairs*, of which there are 676 possibilities from AA to ZZ. These he could have ranked in decreasing order of frequency, assigning a longer code to each as he went down the list. (Had he done this, of course, telegraphers would have given up in despair.) If the 676 letter pairs were coded into binary digits, with no regard to frequencies, the average code length would be about 9.4 bits, doubling the 4.7-bit average code length needed for the simple, twenty-six letter alphabet. On the other hand, if the 676 symbols were coded according to frequency, some commonplace letter pairs (for example, TH and IE) would be assigned codes only two or three bits long, while the least frequent pairs would carry codes sixteen or seventeen bits long. If ordinary English were translated into such a code, a count would show that, on the average, only 7.1 instead of 9.4 bits had been used to encode each pair of letters. This works out to 3.56 bits per letter as against the 4.7 bits required when the code is assigned without *any* reference to frequencies.

The question that fascinated Shannon was how *little* information does ordinary English really contain. If he could determine this

* *In written messages the symbols are the letters of the alphabet; in spoken*

sound wave of the music into a succession of numerical values.

he would know how tightly English might theoretically be encoded. With existing frequency tables he could go only one step beyond two-letter frequencies, to three-letter frequencies (calculated as an aid to cryptographers). These, in Shannon's equation, reduced the code requirement to 3.3 bits per letter.

It is easy to see why no one ever carried the frequency tables beyond three-letter groups: there are 17,576 possible ways to arrange twenty-six letters into groups of three, and nearly half a million combinations of four-letter groups, from AAAA to ZZZZ. Shannon, however, was determined to press further; so he reasoned that any average speaker of English ought to have a tremendous "built-in" knowledge of English statistics. To tap this knowledge, Shannon resorted to ingenious guessing games.

The guessing game

In one game he would pick a passage at random, from a book, and ask someone to guess the letters, one by one. He would tell the subject only if he was wrong, and the subject would continue until he finally guessed the right letter (or space). Shannon quickly discovered that the average person requires substantially fewer than 3.3 guesses to identify the correct letter in ordinary text. The relation between guesses and bits of information should become clearer in what follows.*

One of Shannon's favorite passages for this type of game was "*There is no reverse on a motorcycle a friend of mine found this out rather dramatically the other day.*" In this passage there are 102 letters and spaces, including a final space after "day." Going through the passage letter by letter, one of Shannon's subjects guessed

* [illegible] *Twenty Questions as an exercise in their*

right on his first guess 79 times, and correctly identified all 102 letters and spaces with only 198 guesses, or less than two guesses per letter or space.

In Joyce, a compression?

After mathematical analysis of many such experiments Shannon concluded that in ordinary literary English the long-range statistical effects reduce the information content to about *one bit per letter.* That is to say, if one sees the first 50 or 100 letters of a message, he can be reasonably certain, on the average, that the next following letter (which he hasn't seen) will be one of only two equally probable letters. To remove this much uncertainty requires, by definition, only one bit of information.

Naturally, the amount of uncertainty, hence amount of information, varies among different samples of English. In his basic paper on communication theory, Shannon writes: "Two [opposite] extremes of redundancy in English prose are represented by Basic English and by James Joyce's book, *Finnegans Wake.* The Basic English vocabulary is limited to 850 words and the redundancy is very high. This is reflected in the expansion that occurs when a passage is translated into Basic English. Joyce on the other hand enlarges the vocabulary and is alleged to achieve a compression of semantic content."

Shannon's calculation that the average letter of English (in a long passage) contains only one bit of information has this surprising implication. It says that with proper encoding it should be possible to translate any page of ordinary English into a succession of binary digits, 0 and 1, so that there are no more digits than there were letters in the original text. In other words, twenty-four of the twenty-six letters of the alphabet are superfluous. So far as printed English is concerned, this is the goal that information theory establishes for the communication engineer.*

* *However, the theory recognizes that redundancy often has value. It is English's high redundancy, for example, that makes typographical errors fairly easy*

To help engineers visualize how English might be tightly encoded, Shannon asks them to imagine a communication system in which the transmitting device "guesses" upcoming letters in the way his subject guesses the letters in "*There is no reverse on a motorcycle.*" The numbers under each of the following letters (and spaces) indicate the number of guesses the human subject required for the first words:

T H E R E   I S   N O   R E V E R S E
1 1 1 5 1 1 2 1 1 2 1 1 15 1 17 1 1 1 2

In theory, one could build a transmitter or encoding device that would approach this performance by providing it with a suitable set of operating instructions or program. It might, for example, be programed to guess T to start every message. After T it might always guess H, then E. After E, however, its programed sequence of guesses, in order, might be space, S, I, Y, and R. Presumably the human subject ran through some such sequence before guessing R. Like the human, the machine finds that its program of first and second choices works fine until it reaches the R and V in REVERSE.

With such a transmitter, the symbols that go over the channel are not the letters in the message but the numbers (in binary code) corresponding to the transmitter's guesses. (Naturally, strings of 1's would be coded into more economical form.) The receiver at the other end is an "identical twin" of the transmitter, hence it "knows," for example, that its own fifth guess after T-H-E would be R, and so on.

To reach the goal of two bits per letter—let alone the theoretical one bit—such a transmitter should not even start to guess until it had inspected at least the first ten letters of the message. Once it starts guessing it should make every guess on the basis of probabilities established by the preceding ten letters. This means that

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recognize sixteen of the most important phonetic sounds. When AUDREY recognizes a phonetic sound she signals a hiss-buzz generator (like the Vocoder's) to reproduce it. The result, while not yet equal to the Vocoder product, is surprisingly good. When AUDREY was expected only to recognize digits she frequently had trouble with digits spoken by anyone but her two inventors. Now, however, she can surmount this difficulty. Whereas she may still be unable to recognize a poorly spoken digit, she can signal the hiss-buzz generator to reproduce it accurately enough so it can be recognized as a poorly spoken digit.

If hooked into a phone system, the only signal AUDREY would send over the line would be a four-bit code identifying, in sequence, the particular phonetic sounds uttered by the speaker. Since an average speaker produces about ten of these per second, the channel need have a capacity of only about forty bits per second. This happens to be very close to the capacity needed to send the same message at the same rate by ordinary telegraphy.

Television is predictable

In television the room for improvement is about as great, and the incentive to do something about it is possibly greater. It is much easier to visualize the redundancy in pictures on a TV screen than in a telephone conversation. As in a sequence of movie frames, there is usually very little difference in the successive pictures beamed onto a TV screen. Bell scientists estimate that frequently the redundancy in TV runs as high as 99 per cent. When this happens, the signal, in theory, could be transmitted on a 40,000-cycle, instead of a 4-million-cycle, channel.

Since the Bell System has almost sole responsibility for piping network television around the country, it is more eager than anyone else to slice away at the redundancy. With a compression of perhaps 100-to-1 it might be possible to send television signals across the ocean in one hop as is now done with shortwave radio. Bell

Labs is designing experimental equipment that may achieve the first practical compression of a TV signal.

The battle against noise

The communication art changes rapidly, however, and it is conceivable that ten or twenty years from now channel capacity will be much cheaper than it is today, and the Bell System may no longer feel any urgency about compressing television signals. Reason: Bell has in the laboratory new types of signal-carrying systems that will accommodate—in a single "pipe" called a "waveguide"—hundreds of standard TV channels. Nothing, however, is gained without cost. In the new "pipes" high capacity is achieved by speeding up the signaling process. This makes the signal more vulnerable to imperfections in transmission, which cause what the communication engineer calls noise. The term "noise" covers a host of electric disturbances that degrade a signal. The ultimate unavoidable source of noise is the "thermal" motion of electrons (the hiss) in the electronic equipment itself.

Noise, basically a random phenomenon, is another fundamental problem to which information theory has applied the powerful tools of statistics. The theory tells the engineer how to establish the accuracy with which a message may be transmitted through a noisy channel. As Wiener points out, if there were no noise, the engineer could, in theory, transmit a perfectly measured voltage and thereby transmit an infinity of information. Thus a voltage measured precisely out to the billionth decimal place could represent a coded message a billion digits long.

If the engineer finds he must pass a TV signal through a wide but noisy channel, he will probably turn to a relatively new coding system called "pulse code modulation," or PCM, which minimizes the effects of noise by cutting wave forms into slices and coding the height, or amplitude, of each slice into a series of large, uniform pulses. PCM, in other words, encodes music, pictures, or any

other type of message into the pulse-and-no-pulse of a binary code. PCM is effective in combating noise because it is extremely difficult for noise to give rise to a spurious pulse, or to blur out an existing pulse.

While the first U.S. patent on PCM was issued in 1942, there is reason to believe that PCM's virtues would have gone largely unappreciated if it had not been for information theory. Shannon showed for the first time precisely how the capacity of a channel in bits is related to the bandwidth, the signal power, and the noise. His equation showed that noise could be combated either by raising the signal power, by increasing the bandwidth, or by changing the signaling method. The theory also suggests that signaling methods even better than PCM remain to be discovered.

How *perfect*?

It is the mark of a great theory that, beginning with certain intuitive concepts, it erects a series of relationships, which, rigorously extended, lead to propositions that are not at all self-evident. Thus the intuitive basis of relativity theory would have seemed reasonable to Aristotle, but its conclusion that energy and mass are interchangeable would not.

While information theory does not contain anything so dramatic as $E=mc^2$, it does contain one conclusion of great subtlety that continues to astonish its most diligent students. It is a conclusion that, by extension, has great significance for designers of computers and automatic factories, on the one hand, and neurophysiologists on the other. And there are those who believe it may one day have significance in the everyday (i.e., nonelectronic) affairs of men.

The striking conclusion is this: After setting up the relationship between channel capacity, bandwidth, power, and noise, Shannon goes on to prove that if an information source produces information at a rate that does not exceed the channel capacity, there exists a method for putting the information through the channel

and recovering it at the other side with negligibly small error. Simply stated, this means that a channel, *no matter how noisy*, can give, as a limit, ideal performance—in short, perfection from imperfection. "This to me," says M.I.T.'s Robert Fano, who teaches information theory, "is still a very astonishing thing. It has been definitely proved, but, except for trivial cases, we don't know how to do it."

Evidence is accumulating that living organisms long ago acquired the secret for obtaining near-perfect performance from relatively imperfect apparatus. For example, generations of physiologists have puzzled over the ability of the ear, which seems quite grossly designed, to distinguish two tones almost identical in pitch. Recently, W. H. Huggins obtained his doctorate at M.I.T. with a brilliant theory of hearing that seems to explain the mystery. What the ear employs, evidently, is an extremely clever encoding system. While Huggins made no direct use of information theory, his work is generally included in information theory's fast-growing body of literature, for its speaks of an information-handling mechanism.

Man as communicator

Some of the most interesting applications of information theory, outside of electrical engineering, are being made in experimental psychology. To the psychologist, man may be considered either as a message source or as a channel, but not very readily as a transmitter or a receiver. If you try to measure his abilities purely as a transmitter or receiver, you find you are really using him as a channel. Thus there seems to be no good way to ascertain the rate at which the eye or ear may receive information except by measuring the amount that is remembered or otherwise played back.

In tests run at M.I.T., subjects were asked to point to numbered squares as fast as they could read numbers flashed in random sequence. The test was run with two numbers and two squares,

four numbers and four squares, and so on up to 4,096 numbers and 4,096 squares. As might be expected, the subject can hit quite a few squares per second when he has only a few to choose from, but when he has 1,024 (each worth ten bits) he does well to average 1.5 per second. In terms of information theory, it turns out that the average person can handle about fifteen bits per second.

The highest human channel capacity that M.I.T. psychologists have measured is forty-five bits per second, determined by a variant of the experiment just described. The world's fastest typist, in typing 149 words per minute, is handling just about twenty-five bits per second, if each letter be given a value of two bits. (This seems fair since she probably cannot grasp the long-range clues that, according to Shannon, reduce the information to one bit per letter.) The world's shorthand record is 282 words per minute, which, on the same basis, works out to about forty-seven bits per second.

These figures provide an upper limit for the amount of information a person may handle in a lifetime. The upper limit: roughly 50 billion bits.* One can now appreciate the immense channel capacity used to transmit television. The information handled in the most diligently spent lifetime could, if suitably encoded, be transmitted over a television channel in about sixteen minutes. The information handled in an *average* lifetime could hardly keep a TV channel occupied more than ten seconds.

There are dangers, of course, in overworking any concept, no matter how helpful. Some psychologists who originally encouraged their colleagues to study information theory and to apply it in their experiments now feel that the theory is frequently misapplied by psychologists—and almost inevitably misapplied by sociologists.

Inside the nerve

As Norbert Wiener perceived with his characteristic great enthusiasm, the concepts of information theory apply directly to neuro-

* *Fifty bits per second, twelve hours a day for sixty years.*

physiology. Largely as a result of his inspiration, M.I.T. has become one of the leading centers for the study of the central nervous system. The work, which comes under the Research Laboratory of Electronics, not only has high significance in its own right, but since biological reflexes are the most economical known, it may suggest ways to improve man-made systems.

To learn more of fundamental nerve circuitry, the M.I.T. investigators insert dozens of ultra-tiny electrodes into the spinal cords of anesthetized animals to chart the detailed flow of nerve messages. In the old technique for recording nerve impulses, a relatively large electrode was clamped outside a bundle of nerve fibers. This method, explains one M.I.T. scientist, was about as helpful as trying to analyze the communication network of the entire U.S. using only the signals picked up by ships stationed off the coast. The new electrode-insertion method requires complex electronic recording gear that is available only at a relatively few places like M.I.T., and the work goes slowly.

Information and life

So far no mention has been made of a word that appears in information theory with great frequency. The word is "entropy," and Shannon uses it as synonymous with "amount of information." When Shannon had derived his equation for calculating "amount of information," he found it was precisely the same equation that physicists use to calculate the quantity known as "entropy" in thermodynamics. What the physicist means by "entropy" has stumped freshman physics students for well over seventy-five years, but it is really not too difficult a concept. In thermodynamics, entropy measures the degree of randomness, or disorder, in atomic and molecular systems. The more disorder, the higher the entropy. The famous second law of thermodynamics states that in an isolated system, entropy may stay constant or increase, but never decrease. For an analogy, consider a shoebox into which one puts a handful

of white beads at one end and a handful of black beads at the other. If the box is never touched, the beads will stay in their respective ends, i.e., entropy (disorder) will stay constant. However, the moment the box is disturbed the beads will begin to mix, and disorder, i.e., entropy, will increase.

In Shannon's view, entropy (or amount of information) reaches a maximum when all the symbols in a message appear independently with equal probability, i.e., in random order.* Shannon does not suggest that there is any real identity between his entropy and thermodynamic entropy. Other scientists, however, have speculated that some deep, underlying identity *may* exist.

The identity seems tantalizingly real when one considers the nature of life. Life appears to refute the second law of thermodynamics, until one considers that life cannot continue in a closed system. In his book, *What Is Life?*, Erwin Schrodinger, the Austrian-born physicist, stated a view that has gained popularity when he observed that life feeds on high-grade energy or negative entropy, that is, on substances with highly ordered structures. But the question remains: how does a simple leaf utilize solar energy to erect the primary ordered structures (e.g., sugar, starch, proteins)? It does this, Wiener and others suspect, because photosynthesis employs catalysts that somehow have the power to suspend the second law of thermodynamics, locally and temporarily. Such sorting agents, first proposed by Clerk Maxwell, have been called "Maxwell Demons." For the demon (catalyst) to operate it has to obtain information about the particles it is sorting. If life is thus viewed as a manipulation of energy and information, Wiener and others consider it fitting that both carry "entropy" as a common measure.

* ... prefer to view information ... o them, ... anything

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